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SOME EARLY WORK ON COMBUSTION, RESPIRATION AND CALCINATION.

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WHILE the chemical phenomena of combustion, respiration and calcination have in modern times received an explanation in terms of a common theory, they were for many centuries not even recognized as allied except that, from early times, combustion and respiration were known to depend on air since fire and life both needed air for their maintenance. Hence arose the phrases, *flamma vitalis*, the vital flame, the fire of life and so on, the literary imagery in which a recognition of this simple fact was expressed. But calcination, since it bore no obvious relation to these two processes, and since in contemporary terminology it signified the application of heat to, and its effects on, earthy and mineral bodies in general, not merely the metals, appeared altogether distinct and different.

While this is not intended to be an exhaustive study, we shall find it not uninteresting to consider here some of the early work carried out at the beginning of the modern period on these now admittedly related processes. Man, as we know, had had a long acquaintance with the metals: he had extracted them

from their ores, he had alloyed them and he had turned them to many and various uses in the arts of war and peace. And it was becoming known that on continued heating a metal, though it lost its solidity and was reduced to a powder, nevertheless gained in weight.

The Work of Rey (1630).

One of the earliest and indeed one of the most interesting attempts to explain this increase of weight was made by the provincial French physician, Jean Rey, in his *Essays de Iean Rey Doctevr en Medecine. Svr la Recerche de la cause pour laquelle l'Estain & le Plomb augmentent de poids quand on les calcine* (Bazas, 1630) [1], in which, in a reply to the query of the sieur Brun, Master Apothecary in Bergerac, as to why tin and lead gained in weight on being calcined, he argued that this increase arose from the taking up by the metals of those parts of the air that had been rendered denser than ordinary air in the heating necessary for the calcination. As he naively tells us himself, he spent 'several hours' on the problem, which he describes as 'arduous', and, while he feels that he may be accused of running counter to accepted philosophical maxims, he is satisfied that he cannot be charged either with childish fearfulness or with sordid envy for concealing the truth by keeping his secret to himself; and he adds: 'In any case I shall have testified to the public the desire I have of serving it by letting this paper [2] slip from my hands, although it may be that some hurtful stigma will thereby be graven on my reputation' [3].

Rey's first step is to argue that all things are heavy and that the so-called light elements, air and fire, rise upwards, not by their own natural lightness, but because of the downward motion of the heavier elements, earth and water. There is, he says, no upward movement that is natural; air and fire are heavy and move naturally downwards; and, he adds:

> 'Now since we are agreed that all which sinks downwards without constraint possesses heaviness, whence the movement proceeds, who shall deny this quality to the Air, seeing that as soon as we have dragged a stake out of the ground, it rushes into the hole to fill it? And that we cannot dig a well so deep that the air does not incontinently move thither, without external force, and without violence?' [4]

Weight must be examined by the reason and not by the balance: the former is exact and only employed by the judicious, while the latter is deceptive and used by the greatest clodhopper. Because no element shows weight when weighed in its own medium, it has been wrongly assumed that air is without

[1] An English version of Rey's *Essays* has been published as No. 11 of the *Alembic Club Reprints*, referred to below as *A.C.R.*

[2] A better rendering of the original would be 'this pamphlet'.

[3] *Essays*, 1630, p. 16; *A.C.R.*, p. 6.

[4] *Essays*, 1630, pp. 26–7; *A.C.R.*, p. 10.

weight. Yet, not only has the air weight, but also its weight can be increased by admixture of other heavier matter, by compression and by the removal of lighter parts, and air thus changed will exhibit weight when weighed in air. Now, since water can be made heavier by distilling off its lighter parts, argued Rey, the same must happen to air in heating: the denser parts remain and these will appear heavy when weighed in air. Doubtless, says Rey, there will be some who will say that his examples can be verified in 'our gross and impure air', but that it would be otherwise with pure air. To such critics, he will reply by asking whether they suppose that those who have carried out the experiments and observed the gain in weight that is being discussed have 'procured some purer air, by letters of exchange, from beyond the bounds of nature'[5].

After lingering a moment in his plausible thesis to show that in similar ways air can decrease in weight, Rey gives his answer, which he calls his 'Formal response to the question, why Tin and Lead increase in weight when they are calcined', to the sieur Brun's query as to why, having heated two pounds six ounces of 'fine English tin' in an iron vessel on an open furnace for six hours with continual agitation, he had obtained, without adding anything to it, two pounds thirteen ounces of a white calx, Rey pointing out that an explanation must account also for the loss of weight arising from the increase of volume and for the losses due to vapours and exhalations. He says:

> 'To this question, then, I respond and sustain proudly, resting on the foundations already laid, "That this increase in weight comes from the air, which in the vessel has been rendered denser, heavier, and in some measure adhesive, by the vehement and long-continued heat of the furnace: which air mixes with the calx (frequent agitation aiding) and becomes attached to its most minute particles: not otherwise than water makes heavier sand which you throw into it and agitate, by moistening it and adhering to the smallest of its grains". I fancy there are many who would have been alarmed by the sole mention of this response if I had given it at the beginning, who will now willingly receive it, being as it were tamed and rendered tractable by the evident truth of the preceding Essays. For those without doubt whose minds were preoccupied with the opinion that air was light, would have rushed to oppose it. Why (they would have said) does not one extract heat from cold[6], white from black, light from darkness, since so much heaviness is extracted from air, a thing inherently light? And those who chanced to have bestowed their credence on the heaviness of air, would not have been able to persuade themselves that it can ever increase weight, being balanced in itself. On this account I was constrained to show that air had weight; which was recognisable by other means than the balance: and that even with the latter, a portion previously changed and made denser could manifest its weight'[7].

[5] *Essays*, 1630, p. 90; *A.C.R.*, p. 34.

[6] *A.C.R.*, p. 37, gives 'cold from heat'; *Essays*, 1630, p. 98, reads 'du froid le chaud, le blanc du noir, la clarté des tenebres'.

[7] *Essays*, 1630, pp. 96–8; *A.C.R.*, pp. 36–7.

It has been stated that Rey was not an experimenter, but he does report here a single confirmatory experiment carried out by himself at an iron-works, where he heated a weighed amount of tin on a red-hot pig just poured from the furnace into the mould and found an increase of weight [8]. He did this to show that neither the vapours of charcoal nor its volatile salt could have contributed to the increase, having already shown by argument that neither the disappearance of celestial heat nor the consumption of aerated particles nor the addition of soot or parts of the vessel or attracted humidity could have been the cause. Moreover, the observation of Hamerus Poppius in his *Basilica Antimonii, etc.* (Francoforti, 1618), that antimony calcined by a burning-mirror gained in weight, supported the explanation, Rey regarding this as ' a single experiment ' by which ' all opinions contrary to mine are entirely destroyed ' [9], since he here saw the solar rays acting just as the common fire did in condensing the air ; it refuted every other explanation, and of those who sought to explain the increase in weight as due to attracted humidity, he says ' to seek humidity there, I say, is to seek fire in ice and a knot in a bulrush, as they say, a thing which never can be found ' [10]. As for there being a limit to the increase in weight, nature had here set bounds and never overstepped them ; the calx became saturated and could take up no more condensed air, just as solids could take up only limited amounts of liquids. Further, the fact that some calces and ashes did not exhibit an increase in weight might be an objection to his theory, until it was realized that they might even show a decrease in cases where the loss through matter being driven off by the fire or through increased volume was not counterbalanced or rather exceeded by the addition of the heavy parts of the air ; and in similar ways, contradictory results had been obtained with lead when the impure metal had been used, the sieur Brun, for example, finding a decrease of one ounce in the pound, whereas pure lead, like tin, gained in weight on calcination [11].

Rey and Mersenne (1634).

The lack of knowledge of Rey's work by his contemporaries is curious, especially since he conducted a correspondence with Mersenne [12], in which the latter pointed out that, if Rey was correct, gold and silver ought to show similar gains in weight, whereas they did not [13]. Moreover, in 1634 Mersenne published his *Qvestions Theologiqves, etc.*[14], a work containing various questions

[8] *Essays*, 1630, pp. 124–5 ; *A.C.R.*, p. 47.
[9] *Essays*, 1630, pp. 130–6 ; *A.C.R.*, pp. 49–51.
[10] *Essays*, 1630, p. 135 ; *A.C.R.*, p. 51.
[11] *Essays*, 1630, pp. 140–3 ; *A.C.R.*, pp. 53–4.
[12] Reproduced in Gobet's reprint of Rey's *Essays*, Paris, 1777, pp. 107 ff.
[13] *Ibid.*, p. 147.
[14] *Les Qvestions Theologiqves, Physiqves, Morales, et Mathematiqves. Où chacun trouuera du contentement, ou de l'exercice. Composees par L.P.M.*, Paris, 1634. The dedicatory *Epistre* is signed ' F. M. Mersene M.'

as to what are the chief human curiosities, why some think themselves *sçauans* and others who deserve such a title consider themselves ignorant, why les *sçauans* do not usually attain such great dignities as *les vaillans*, why most men prefer lucre and utility to science and honesty, and other questions such as the title would suggest; and in *Qvestion III*, 'Est-il vray que l'Estain calciné, est plus pesant apres auoir esté calciné, que lors qu'il est crud', and its corollary (pp. 11–16) he refers to Rey's explanation and says that he prefers it to all others and that, although many difficulties could be raised against it, Rey had resolved most of them [15]. Rey's work was brought into considerable notice in 1775 [16] immediately after Lavoisier had published the earlier version of his first memoir on the calcination of metals (lead and tin) in sealed vessels [17]: and while it is often stated that it had been lost meanwhile, various writers do actually refer to it so that it appears to have been ignored rather than forgotten [18].

Boyle's Theory of Calcination (1673).

But another explanation, advanced by Boyle in his *Essays of Effluviums* (London, 1673) [19], found considerable and extensive favour. Boyle calcined copper, silver, tin, lead, steel, zinc and other substances by various methods, in crucibles covered with other crucibles, in crucibles luted one upon another, in sealed glass vessels and in glass vessels closed with corks or with a 'Cap' or 'Stopple' of paper, over the flame of sulphur, on a cupelling furnace, in a sand furnace, on charcoal and by the flame of spirit of wine. The increments in weight that he observed he ascribed to the addition of the effluvia or fine particles of fire. His examination of the problem was very thorough: for instance, he carried out a blank experiment to show that the gains in weight were not due to matter derived from the vessels. It is to be noted that he weighed the product by removing it from its container, not weighing the whole sealed apparatus before and after heating. The experiments in sealed glass vessels, carried out thus to prevent access of smoke or other particles, proved dangerous; at the first attempt, shortly after the sealed retort was placed on the fire, 'it suddenly broke in a great multitude of pieces, and with a noise like the Report of a Gun; but (thanks be to God) it did no harm neither to me nor others that were very near it'. But, in seeking to avoid such unsuccessful issues, Boyle was led very unfortunately to modify his procedure in the wrong way: thereafter, when he had placed the metal in the vessel, he did not seal

[15] Gobet (*op. cit.*, pp. 176–81) reproduces this from Mersenne, but without bibliographical details. He gives merely 'une question du Pere Mersene, sur la même matière, imprimée en 1634' (*ibid.*, p. xj).

[16] By Bayen in Rozier's *Observations sur la Physique*, 1775, **5**, 47.

[17] *Observations sur la Physique*, 1774, **4**, 446.

[18] *Cf.* the author's *Antoine Lavoisier*, London, 1935, pp. 196–7.

[19] An analysis of these *Essays* has been given by the author in *Science Progress*, 1934, **29**, 253.

the latter off until he had heated it for some time to allow for the expansion of air, and when he subsequently broke the seal the noisy inrush of the air was to him merely a proof that the vessel had been properly sealed. But in this preliminary heating, as he sometimes observed, some ' fumes ' or ' smoak ' escaped. However, once more he found a gain in weight, and, as before, he removed the products from the apparatus before weighing them.

To Boyle, flame was hot rarefied fuel ; its parts united with the parts of bodies exposed to its action and, he says, his experiments in the sealed vessels showed ' that what is afforded by Fire may in a Corporeal way invade, adhere and add Weight to even fixt and ponderous Bodies '. Moreover, as he noted in the case of lead, although ' the *absolute* Gravity ' was increased, ' yet this Aggregate of Lead and extinguish'd Flame had lost much of its *specifick* Gravity ' ; the particles had not been transmuted into metal, but had made ' a Coalition with those of the Lead ', or, in other words, the calx was a compound body. Some, but only the finest, the most minute and most active, of the particles of flame or igneous corpuscles must have passed through the pores of the glass, and this only when its texture had been opened by the violence of the fire.

Boyle also pointed out that his results contradicted the accepted view that in calcination the greater part of the calcined body was driven off, leaving only its earth and fixed salt ; for no moist or fugitive parts appeared to be expelled in the process, and, moreover, he had obtained lead by merely heating its calx, which showed that calcination had not deprived it ' of the suppos'd radical moisture requisite to a metal '.

Throughout the *Essays* Boyle was concerned with exhibiting the superiority of the ' corpuscular Philosophy ' in explaining the properties of bodies over the Aristotelian four-element theory and the Scholastic doctrine of substantial forms, and also with showing that the so-called ' occult ' properties could find readier and more satisfactory expression in particulate explanations. Indeed, there seems a very strong probability that he had already evolved this explanation before he set about his experiments, since in his *Sceptical Chymist* of 1661 he had already noted, he says from experiments, that ' the Particles of an open Fire working upon some Bodies may really Associate themselves therewith, and add to the Quantity ' [20].

Boyle and Chérubin (1679).

Boyle was criticized in 1679 by Chérubin [21], who disputed the penetrability of glass even as envisaged by Boyle, who had limited it to very fine particles

[20] *The Sceptical Chymist*, London, 1661, p. 212. Everyman edition, p. 119.

[21] *Effets de la Force de la Contiguité des Corps*, Paris, 1679, *Appendice*, ' De l'Impermeabilité du Verre, où est demontré, que le Verre brut, n'a point de pores, qui penetrent ses deux superficies ', pp. 343–62. Chérubin's work has been discussed in detail by the author in *Science Progress*, 1936, **31**, 55.

violently moved by a fire strong enough to open the texture of the glass. Chérubin pointed out that the inrush of air that Boyle heard in opening the sealed vessel after heating proved the glass impenetrable ; furthermore, Boyle ought to have weighed the retort intact with all its contents before opening it and then weighed it again to discover whether the air that had entered had caused any change in weight, and he could have concluded that glass was pervious only if these weights were identical. Without this, his theory was not positively proved. If Boyle had done this, Chérubin added, he would have found the second weight greater than the first, since calcined bodies strongly attracted air, especially its moist parts, the addition of which was evidently the cause of the increments observed by Boyle, just as Digby [22] had observed similar increases in salt of tartar exposed to the air.

Boyle, in a letter [23], for which both the date and the name of the person to whom it was addressed are missing, stated that this possible objection had been made to him, but that he had weighed a sealed retort in the manner suggested by Chérubin with matter in it and had found that it increased in weight. And as for Digby, was it not he who had asserted that he had made quicksilver pass in small drops through glass itself ? This was neither the kind of statement that the cautious Boyle was ready to lay much weight on nor the kind of witness whose credibility he would have any great regard for. And Chérubin, he says, might allow one, ' that is a piece of a chemist ', to remark that calcined bodies differ in kind, some taking up moisture, others, like the calces of metals, not doing so ; and besides it did not seem probable that the metals could acquire, in the interval of a few minutes between opening the retort and weighing it, the increments of weight that he had observed.

Some other Theories of Calcination (1667–1747).

Meanwhile, the phlogiston theory arose ; and, on calcination, metals were alleged to lose their phlogiston. The gain in weight was, however, frequently explained by means of Boyle's theory. But other explanations arose from time to time. Du Clos [24] (1667) ascribed the increments to sulphureous particles from the air, Homberg [25] (1705) to the fixation of the matter of light [26], which was the ' sulphur ' principle or element, Hartsoeker [27] (1706) to the

[22] Sir Kenelm Digby, *A Late Discourse made in a Solemne Assembly of Nobles and Learned Men at Montpellier in France*, trans. R. White, London, 1658, pp. 36–7 and 66.

[23] *Works*, ed. Birch, London, 1772, vi, 41–2.

[24] Du Hamel, *Regiæ Scientiarum Academiæ Historia*, Paris, 1698, p. 13.

[25] *Mém. Acad. R. Sci.*, 1705, p. 94.

[26] Boyle states in the *Essays of Effluviums*, referred to above, that he had intended to carry out experiments with burning-glasses on the calcination of metals, whence we may expect that he had hoped to prove that light was ' corporified ' in bodies, but the ' cold and wet Summer ' was ' unfriendly to the Tryals '.

[27] *Conjectures Physiques*, Amsterdam, 1706, p. 129 ; *Eclaircissemens sur les Conjectures Physiques*, Amsterdam, 1710, pp. 68–70.

heterogeneous, saline and other, particles in the air or the fire or in both. And Chérubin [28] in 1679 had ascribed them to absorption of atmospheric moisture, while in 1727 Hales [29] said they were due to air lodged in the metal in combination with the sulphureous parts of the fuel. In 1747 Béraut [30] had, while refuting fire or air itself as possible sources, argued that the gains arose from precipitation of the foreign matter in the air deposited by the rarefying action of the fire on the neighbouring air, the air thus rarefied being no longer able to sustain its customary burden—unaware that the same objection could be laid against this as that brought forward by Mersenne [31] against Rey's theory, namely, that gold and silver should show similar increments, whereas they did not.

The gain in weight of metals on calcination thus remained a problem ; and the historian [32] of the Paris Academy wrote in 1766 in referring to the work of Tillet, read before the Academy in 1763 :

> ' It is a true chemical paradox which experiment however puts beyond all doubt ; but, if it is easy to prove the fact, it is not so easy to give a satisfactory reason for it ; it escapes all our physical theories, and we can only leave to time the solution of this difficulty " [33].

Eight years later, in November 1774, Lavoisier read his first memoir on calcination before the Paris Academy ; the historian of the Academy, admittedly writing later than 1774 since the volume for 1774 did not appear until 1778, very properly noted that ' air, for long regarded in this process as a necessary but purely mechanical agent, has become the necessary chemical agent ' [34].

Boyle's Experiments on Combustion (1660).

Turning to consider the prosecution of the experimental study of the problems of combustion and respiration, we find that Boyle carried out numerous experiments to this end before 1660 in his ' Pneumatical Engine ', as he called the air-pump constructed for him in 1658–9 by Robert Hooke. The experiments on combustion included the placing of lighted candles and wax tapers in the receiver of the air-pump, the receiver being thereupon evacuated of its

[28] *Cf.* pp. 148–9 above.

[29] *Vegetable Staticks*, London, 1727, pp. 286–7.

[30] *Dissertation sur la Cause de l'Augmentation de Poids, que certaines Matières acquièrent dans leur Calcination*, Bordeaux, 1747. Béraut's work has been discussed in detail by the author in *Annals of Science*, 1936, **1**, 269.

[31] *Cf.* pp. 146–7 above.

[32] De Fouchy.

[33] *Hist. Acad. R. Sci.*, 1763, pp. 47–8 (published 1766).

[34] *Hist. Acad. R. Sci.*, 1774, p. 21 (published 1778) : ' . . . . l'air regardé long-temps dans cette opération comme un agent nécessaire, mais purement mécanique, y devient nécessaire comme agent chimique '.

air as rapidly as possible; the flame was in all cases soon extinguished, whereas it was observed that in the closed but unevacuated receiver it continued to burn much longer [35]. In another experiment Boyle found that 'kindled Wood-coals' were extinguished in the evacuated receiver in less time than in the unevacuated receiver, though they were rekindled if removed and swung about in the open air, and that they continued to burn in the open air for a much longer time than in either of the experimental conditions, whence it appeared that the removal of the air made the fire go out sooner than it would otherwise have done, 'though that part of the Air that we drew out left the more room for the stifling steams of the Coals to be received into' [36]. Again, 'a piece of Match, such as Souldiers use', ignited and placed in the receiver, appeared to be completely extinguished upon evacuation, but rekindled when air was re-admitted [37]. Then, 'a lighted piece of Match' was placed in the receiver together with 'a great Bladder well tyed at the Neck, but very lank, as not containing actually much (if any thing) above a Pint of Air, but being capable of containing ten or twelve times as much', of which Boyle says:

> 'Our scope in this Experiment was partly to try whether or no the smoke of the Match, replenishing the Receiver, would be able to hinder the Dilatation of the inward Air, upon the exsuction of the Ambient. And partly to discover whether the extinction of the Fire in the Match did proceed from want of Air, or barely from the pressure of its own Fumes, which for want of room to expand themselves in, might be suppos'd to Recoyl upon the Fire, and so to stifle it';

on evacuation, the match was slowly extinguished, but if air was allowed to enter before extinction was complete the fire revived, it being noted also at the same time,

> 'That even when the Fumes seemed most to replenish the Receiver, they did not sensibly hinder the Air included in the Bladder from dilating it self after the same manner (for ought we could discern) as it would have otherwise done: So that before the Fire or the Match was quite extinct, the Bladder appear'd swell'd at least to six or seven times its former capacity' [38].

Attempts to fire the gunpowder in the pan of a pistol included in the evacuated receiver, by operating the trigger from outside, were not very successful, but Boyle succeeded once in firing gunpowder in this way *in vacuo* [39]. He failed,

[35] *New Experiments Physico-Mechanicall, Touching The Spring of the Air, and its Effects (Made, for the most part, in a New Pneumatical Engine), etc.*, Oxford, 1660, Experiment 10, pp. 74–8.

[36] *Ibid.*, Experiment 11, pp. 78–82.

[37] *Ibid.*, Experiment 12, pp. 82–3.

[38] *Ibid.*, Experiment 13, pp. 84–8.

[39] *Ibid.*, Experiment 14, pp. 88–102 (pp. 90–9 omitted in pagination).

however, to ignite combustibles in the evacuated receiver from outside by means of a burning-glass [40].

Upon this interesting series of experiments, Boyle stated that he forbore for the moment to make any reflections [41].

Boyle's Experiments on Respiration (1660).

Later, in the same work, Boyle describes other experiments, or rather a lengthy experiment [42], concerned with respiration. A lark, a sparrow or a mouse (' newly taken, in such a Trap as had rather affrighted than hurt him '), placed in the receiver, rapidly expired on the ex-suction of the air, although the evacuation of the air involved also the evacuation of the ' fuliginous Steams ', discharged from the lungs in expiration, ' which in the other cases hitherto known, may be suspected, for want of room, to stifle those Animals that are closely pent up in too narrow Receptacles '. It therefore appeared that these creatures had died from want of air, not because the air was ' overclogg'd by the steams of their Bodies, exquisitely pent up in the Glass '. Boyle accordingly proceeded to confirm this conclusion by further trial :

> ' Nor was I content with this, but . . . . I caus'd a Mouse, that was very hungry, to be shut in all Night, with a Bed of Paper for him to rest upon : And to be sure that the Receiver was well clos'd, I caus'd some Air to be drawn out of it, whereby, perceiving that there was no sensible leak, I presently re-admitted the Air at the Stop-cock, lest the want of it should harm the little Animal ; and then I caus'd the Engine to be kept all Night by the Fire side, to keep him from being destroy'd by the immoderate cold of the Frosty Night. And this care succeeded so well, that the next Morning I found that the Mouse not onely was alive, but had devour'd a good part of the Cheese that had been put in with him. And having thus kept him alive full twelve hours, or better, we did, by sucking out part of the Air, bring him to droop, and to appear swell'd ; and by letting in the Air again, we soon reduc'd him to his former liveliness ' [43].

Thus the mouse had lived in the receiver overnight, for hours instead of minutes, despite the ' fuliginous Steams ' he continually expired, in an amount of air equal to that whose evacuation from the receiver had rapidly killed the mice and other creatures included in it.

After he had completed these experiments, Boyle turned to set down his thoughts on the problem of respiration in ' A Digression containing some Doubts touching Respiration ' [44]. He acknowledged that his readers might expect him to add his reflections on the results that he had already obtained in an

[40] *Ibid.*, Experiment 15, pp. 102–5.
[41] *Ibid.*, p. 104.
[42] *Ibid.*, Experiment 41, pp. 328–34.
[43] *Ibid.*, p. 334.
[44] *Ibid.*, pp. 335–83.

inquiry so deliberately designed. But he felt that he had made too few experiments and those had, he said, ' made Respiration appear to me rather a more, than a less Mysterious thing, then it did before ' [45]. Indeed, they had confirmed his ' Diffidence of the Truth of what is commonly believ'd touching that matter ' ; and therefore, as a true disciple of the ' great Verulam ', he set forth his doubts, presented some hints and discussed ' whether the Tryals made in our Engine, will at least assist us to discover wherein the Deficiency lies that needs to be supply'd ' [46].

Accordingly, after various lengthy references to the anatomical observations of others and to numerous contradictory attempts to explain the purpose of respiration, Boyle concluded that, although facts and opinions inclined him ' to look upon the Ventilation and Depuration of the Blood, as one of the principal and constant uses of Respiration ', yet ' methinks it may be suspected that the Air does something more than barely help to carry off what is thrown out of the Blood in its passage through the Lungs, from the right Ventricle of the Heart to the left ' [47]. For in many diseases, the blood circulated ' tolerably well, notwithstanding its being excessively serous ', and asthmatical persons, ' though the Lungs be very much stuff'd with tough Phlegm ', lived for months and even years, whence Boyle argued :

> ' So that it seems scarce probable, that either the want of throwing out the superfluous *Serum* of the Blood for a few Moments, or the detaining it, during so short a while, in the Lungs, should be able to kill a perfectly sound and lively Animal : I say, for a few moments, because, that having divers times try'd the Experiment of killing Birds in a small Receiver, we commonly found, that within half a minute of an hour, or thereabouts, the Bird would be surpris'd by mortal Convulsions, and within about a minute more would be stark dead, beyond the Recovery of the Air, though never so hastily let in ' [48].

The theoretical explanations and current contemporary opinions that Boyle had previously quoted apparently offered him little scope for experimental attack with the resources at his disposal ; the most that he could do was to counter opinion with contradictory fact, the ' fact ' frequently being some unusual physiological appearance or some recent experiment vouched for by another writer ; but his own experiments on the death of animals in the evacuated receiver of his air-pump at last enabled him to make a better attack on the problem, since, from an opinion expressed by Paracelsus, they gave him a clue as to what question he should ask Nature. After confirming his previous results, he says :

> ' If to the foregoing Instances of the sudden destruction of Animals, by the removal of the ambient Air, we should now annex some, that we

[45] *Ibid.*, p. 335.
[46] *Ibid.*, p. 336.
[47] *Ibid.*, p. 359.
[48] *Ibid.*, pp. 359–60.

> think fitter to reserve till anon ; perhaps Your Lordship [49] would suspect, with me, that there is some use of the Air, which we do not yet so well understand, that makes it so continually needful to the Life of Animals. *Paracelsus* indeed tells us, *That as the Stomack concocts Meat, and makes part of it useful to the Body, rejecting the other part, so the Lungs consume part of the Air, and proscribes the rest.* So that according to our Hermetick Philosopher (as his followers would have him stil'd) it seems we may suppose, that there is in the Air a little vital Quintessence (if I may so call it) which serves to the refreshment and restauration of our vital Spirits, for which use the grosser and incomparably greater part of the Air being unserviceable, it need not seem strange that an Animal stands in need of almost incessantly drawing in fresh Air. But though this Opinion is not (as some of the same Author) absur'd, yet besides that, it should not be barely asserted, but explicated and prov'd ; and besides that, some Objections may be fram'd against it, out of what has been already argu'd against the Transmutation of Air into vital Spirits : Besides these things, it seems not probable, that the bare want of the Generation of the wonted quantity of vital Spirits, for less then one minute, should within that time be able to kill a lively Animal, without the help of any external violence at all ' [50].

Paracelsus's opinion reminded Boyle that Drebbel, who was said to have contrived a vessel that would go under water, had, according to such inquiries as Boyle had been able to make, thought that it was not the whole of the air, but only a certain quintessence or spirituous part of it that made it fit for respiration, and that, when once this part was spent, the remainder was ' unable to cherish the vitall flame residing in the heart ', whence he used a ' Chymicall liquor, which he accounted the chief Secret of his submarine Navigation ', unstopping its container when the finer and purer part of the air was consumed or ' overclogg'd ' by the respiration and ' steams ' of the crew and thus speedily restoring to the air the vital parts necessary to make it respirable for some time. Boyle adds that he himself had been much inclined to the opinions of those who held that air was necessary ' to ventilate, and cherish the vitall flame, which they do suppose to be continually burning in the heart ' ; for experiments with the ' Engine ' showed that the flame of a lamp lasted as little after the evacuation of the air as did the life of an animal—' a new kind of resemblance betwixt fire and life ' [51].

After some experiments on the pulsation of the heart of a puppy removed from a bitch immediately after death, and a suggestion that fishes might use in respiration, in the passage of water through their gills, the ' interspers'd Air ' which his experiments had shown water to contain, Boyle placed a large eel

[49] The book was ' Written by way of Letter To the Right Honorable *Charles* Lord Vicount of *Dungarvan* ', Boyle's nephew.

[50] *Ibid.*, pp. 362–3.

[51] *Ibid.*, pp. 363–7.

in the receiver of the air-pump; on evacuation of the air, the eel seemed ' somewhat dis-compos'd ' and presently lay motionless as if dead;

> ' and though I did not think her so, yet the continuing in that Posture, even after the Cover of the Receiver was taken off (whereby the Air was let in) I should have been of the Opinion of the By-standers, if the Diffidence I am wont to exercise in trying Experiments (especially such as are not usual) had not invited me to take the Fish out of the Receiver, upon which she shew'd herself, by her vivid motions, as much alive as before ' [52].

He remarked here that the fœtus might respire in the womb as a fish in water, and while he wondered ' whether it be not practicable, to make a Receiver, though not all of glasse, yet with little glasse windows, so placed, that one may freely look into it, capacious enough to hold a Man ' [53], he experimented meanwhile with insects such as ' an humble-bee', a ' flesh fly ' and a ' Palmer worme '. On removal of the air, all seemed dead within a minute, but showed signs of life on the re-admission of air [54]. While it was known that bees and flies would not only walk, but even fly for some considerable time after decapitation, yet, upon removal of air, the motions of their bodies and even of their limbs ceased, ' as if the presence of the Air were more necessary to these Animals, then the presence of their own Heads ' [55].

For the moment, Boyle postponed speculation; his object, he said, had been to collect facts towards a further investigation, but, beyond the possibility that the air sometimes cooled and condensed the blood in the lungs, and while the ' depuration ' of the blood in its passage through the lungs was one of the principal uses of respiration, it seemed to him that ' the Air does something else in Respiration, which has not yet been sufficiently explain'd ' ; and therefore he preferred, with ' the acute St. *Austin* ', to confess cautious ignorance than to profess false knowledge (*magis eligo cautam ignorantiam confiteri, quam falsam scientiam profiteri*) [56].

Boyle's further Experiments on Respiration (1670).

In 1670 Boyle in continuing his studies on respiration confirmed the necessity of air to the life of animals in some further interesting experiments [57] in which, after finding that various animals died a little sooner by drowning than by the removal of the air surrounding them in the receiver of his ' Engine ', and after noting that in drowning the destructive action was applied at once, whereas in the receiver of the air-pump it was but gradual, he successfully

[52] *Ibid.*, p. 372.
[53] *Ibid.*, p. 375.
[54] *Ibid.*, pp. 377–8.
[55] *Ibid.*, p. 381.
[56] *Ibid.*, p. 383.
[57] *Phil. Trans.*, vol. v, no. 62, August 8, 1670, pp. 2011–31.

killed a mouse almost immediately by the sudden removal of nearly all the air in which it was confined. He says :

> ' But this one thing would be taken notice of, that, though some of the abovementioned Animals seem, by the Relations we have given of them, to have been a little sooner destroyed by drowning, than any we have mentio'nd were by our Engin, that is no sure proof, that suffocation does kill Animals faster, than the deprivation of Air, they are exposed to in our Engin. For in drowning, that which destroys is applyed to its full vigour at the very first, and all at once ; whereas, our Receivers being made for several purposes, the Deprivation of the Air, that they make, cannot be made all at once, but the Air must be pump't out by degrees ; so that till the last the Receiver will be but partly emptied. For confirmation of which, I have this to alledge, that, having in the presence of some *Virtuosi* provided for the nonce a very small Receiver, wherein yet a Mouse could live sometime, if the Air were left in it, we were able to evacuate it at one suck, and by that advantage we were enabled, to the wonder of the Beholders, to kill the Animal in less than half a minute ' [58].

In another paper, published in the same year [59], Boyle described how he had placed a mouse and a mercury gauge in a sealed glass vessel and had found that the respiration of the mouse, continued until it was apparently quite dead, produced no sensible change in the pressure of the air, a similar result being obtained with a small bird. Boyle reports his result as follows, under the title ' Some Experiments showing, that Air, become unfit for Respiration, may retain its wonted pressure ' :

> ' We took a Mouse of an ordinary size, and having (not without some difficulty) conveyed him into an Ovall Glass, fitted with a somewhat long and considerably broad neck, which we had provided, that it might be wide enough to admit a Mouse in spight of his struggling. We conveyed in after him a Mercurial Gage, in which we had diligently observed and marked the Station of the Mercury, and which was so fastned to a Wire reaching to the bottom of the Oval Glass, that the Gage, remaining in the neck, was not in danger to be broken by the motions of the Mouse in the Oval part : The upper part of the long neck of the Glass was, notwithstanding the wideness of it, hermetically sealed by the help of a Lamp and a pair of Bellows, that we might be sure, that the imprisoned Animal, should breath no other Air, then that which filled the Receiver at the time when it was nipped up. This done, the Mouse was watched from time to time, and though by reason of the largeness of the Vessel in comparison of so small an Animal, he seemed to me rather drooping then very near death at the end of the second hour ; yet coming to look upon him about half an hour after, he was judged by the Spectators quite dead, notwithstanding our shaking of the Vessel to rouze him up. This made me cast my eyes upon the Gage, wherein I could not perceive any sensible change of the Mercuries Station ' [60].

[58] *Ibid.*, p. 2031.
[59] *Ibid.*, no. 63, Sept. 12, 1670, pp. 2035–56.
[60] *Ibid.*, pp. 2046–7.

Boyle on Flame, Air and Respiration (1672).

Then in his *Tracts . . . . Containing New Experiments, touching the Relation betwixt Flame and Air, etc.* (Oxford, 1672), Boyle, after a long series of experiments on the difficulty of producing, preserving and propagating flame without air, came, in other experiments on the relation between air and the *flamma vitalis* of animals, upon a fact that proved a great obstacle, then and later, to the correlation of the phenomena of combustion with those of respiration ; for he found that animals placed in closed vessels of air together with inflamed substances easily survived the extinction of the flames, even when the flames were put out by the gradual evacuation of the air from the vessels. Of this experiment Boyle says :

> ' . . . . we took the small Glass-lamp . . . ., and having lighted it, we plac'd both it and a small Bird (which was a Green-finch) upon the Brass-plate, and in a trice fastned it to the lower orifice of the Receiver, and then watched the event ; which was, that within two minutes (as near as we could estimate by a good minute-watch) the flame, after having several times almost quite disappear'd, was utterly extinguished ; but the Bird, though for a while he seem'd to close his eyes as though he were sick, appear'd lively enough at the end of the third minute ; at which time, being unwilling to wait any longer by reason of some avocations, I caused him to be taken out.
>
> ' After he had for a pretty while, by being kept in the free Air, recovered and refreshed himself, the former tryal was repeated again, and at the end of the second minute the flame of the Lamp went out ; but the Bird seem'd not to be endanger'd by being kept there a while longer.
>
> ' After this we put in together with the same Bird two lighted Lamps at once, (*viz.* the former and another like it) whose flames, according to expectation, lasted not one whole minute before they went out together. But the Bird appear'd not to have been harmed, after having been kept five or six times as long before we took off the Receiver ' [61].

These results were confirmed in experiments of a similar kind with a mouse both with the lamp and with a wax-candle. The experiments in which the air was gradually evacuated are reported by Boyle as follows :

> ' We took a Green-finch and a piece of Candle of twelve to the pound, and included them in a great capp'd Receiver, capable of containing about two Gallons or sixteen pound of water, which was very carefully cemented on to the Pump, that no Air might get in or out. In this Glass we suffer'd the Candle to burn till the flame expired, (which it did, in more than one Tryal, within two minutes or somewhat less ;) at which time the Bird seemed to be in no danger of sudden death ; and, though kept a while longer in that clogg'd and smoaky Air, appeared to be well enough, when the Receiver was removed. Afterwards, we put the same Bird into the Receiver with a piece of a small wax Taper, whose flame though it lasted longer than the other, yet the Bird outlived it ; and 'twas judged he would have done so, though the Flame had been much more durable.

[61] *Op. cit.*, pp. 110–12.

> After this, we included the same Bird with the first-mention'd Candle in the Receiver, which we had caused to be often blown into with a pair of Bellows, to drive out the smoak and infected Air ; and then beginning to pump out the Air, we found, that the Flame began more quickly to decay, and the Bird to be much more discomposed than in the former Experiments ; but still the *Animal* outlived the Flame, though not without Convulsive motions ' [62].

Further similar experiments confirmed this result. Boyle's comment was :

> ' Whether this survival of Animals, not only to a flame that emits store of fuliginous steams, as in this tryal ; but to that which is made of so pure a fuel as Spirit of Wine, that affords not such steams (as in the former experiment ;) *Whether*, I say, this survival proceed from this, That the Common flame and the Vital flame are maintained by distinct substances or parts of the Air ; *or* that common Flame making a great waste of the Aereal substance, they both need to keep them alive, cannot so easily as the other find matter to prey upon, and so expires, whilst there yet remains enough to keep alive the more temperate Vital flame ; *or* that both these causes, and perhaps some other, concurr to the *Phænomenon*, I leave to be consider'd ' [63].

Boyle's Theory of Combustion and Respiration (1674).

Shortly afterwards in his *Tracts : Containing I. Suspicions about some Hidden Qualities of the Air ; etc.* (London, 1674), Boyle emphasized his belief in the heterogeneity of atmospherical air ; to him ' perhaps there is scarce a more heterogeneous Body in the world ' [64]. Although it seemed wholly unfitted to be a menstruum, ' yet ', he says, ' I am not sure but it may have a dissolving, or at least a consuming, power on many Bodies, especially such as are peculiarly dispos'd to admit its operations ' [65] : and here Boyle gave expression to what later was recognized as a most important principle in chemical dynamics :

> ' For I consider, that the Air has a great advantage by the vast Quantity of it, that may come to work in proportion to the Bodies that are expos'd to it : And I have long thought, that, in divers cases, the Quantity of a *Menstruum* may much more considerably compensate its want of strength, than Chymists are commonly aware of ' [66].

But it was here at last that Boyle set out his speculations on combustion and respiration and their connexion with air. He says :

> ' The Difficulty we find of keeping Flame and Fire alive, though but for a little time, without Air, makes me some times prone to suspect, that there

[62] *Ibid.*, pp. 114–16.
[63] *Ibid.*, pp. 117–18.
[64] *Op. cit.*, p. 2.
[65] *Ibid.*, p. 6.
[66] *Ibid.*, pp. 6–7.

may be dispers'd through the rest of the Atmosphere some odd substance, either of a Solar, or Astral, or some other exotic, nature, on whose account the Air is so necessary to the subsistence of Flame; which Necessity I have found to be greater, and less dependent upon the *manifest* Attributes of the Air, than Naturalists seem to have observed. For I have found by tryals purposely made, that a small flame of a Lamp, though fed perhaps with a subtil thin Oyl, would in a large capacious glass-Receiver expire, for want of Air, in a far less time than one would believe. And it will not much lessen the difficulty to alledge, that either the gross fuliginous Smoak did in a close Vessel stifle the flame, or that the pressure of the Air is requisite to impel up the aliment into the wieck: For, to obviate these objections, I have in a larger Receiver imploy'd a very small wieck with such rectified Spirit of Wine, as would in the free Air burn totally away; and yet, when a very small Lamp, furnished (as I was saying) with a very slender wieck, was made to burn, and, fill'd with this liquor, was put lighted into a large Receiver, that little flame, though it emitted no visible smoak at all, would usually expire within about one minute of an hour, and, not seldom, in a less time; and this, though the wieck was not so much as sing'd by the flame: Nor indeed is a wieck necessary for the experiment, since highly rectified Spirit of Wine will in the free Air flame away well without it. And indeed it seems to deserve our wonder, what that should be in the Air, which inabling it to keep flame alive, does yet, by being consum'd or deprav'd, so suddenly render the Air unfit to make flame subsist. And it seems by the sudden wasting or spoiling of this fine Subject, whatever it be, that the bulk of it is but very small in proportion to the Air it impregnates with its virtue. For after the extinction of the flame, the Air in the Receiver was not visibly alter'd, and, for ought I could perceive by the ways of judging I had then at hand, the Air retain'd either All, or at least far the greatest part of its *Elasticity*, which I take to be its most genuine and distinguishing property.

' And this *undestroy'd springyness* of the Air seems to make the necessity of fresh Air to the Life of *hot* Animals, (few of which, as far as I can guess after many tryals, would be able to live two minutes of an hour, if they were totally and all at once deprived of Air,) suggest a great suspicion of some *vital substance*, if I may so call it, diffus'd through the Air, whether it be a *volatile Nitre*, or (rather) some *yet anonymous* substance, Sydereal or Subterraneal, but not improbably of kin to that, which I lately noted to be so necessary to the maintenance of other flames ' [67].

This theory, that there was something in the air that nourished both fire and life, was also expressed by Hooke and Mayow, as we shall note presently, while Newton in no less a work than the *Principia* also held that there was in air a ' subtle and useful part ' sustaining ' the life of all things with us '.

Boyle's later Experiments on Combustion and Respiration (1680).

In 1682 in *A Continuation of New Experiments Physico-Mechanical Touching the Spring and Weight of the Air, And their Effects. The Second Part: Wherein Are contained divers Experiments made both in compressed and also in factitious*

[67] *Ibid.*, pp. 24–27.

Air, about Fire, Animals, &c., etc. (London, 1682), the first edition of which had appeared in Latin in 1680 [68], Boyle reported experiments carried out between March 1676, and February, 1679, in which he found that ' Artificial ' or ' Factitious Air ', from fermenting bread or paste or fruits or from the action of distilled vinegar on pulverized coral, was noxious to the life of animals, such as bees, flies, birds, frogs, mice, snails and an adder, unless mixed with common air, when it was not so hurtful [69]. In fact, it seemed to be a greater enemy to them than a vacuum was [70]. Similarly, it proved ' prejudicial to Fire ' [71]. In the experiments with animals, it seemed to have ' some venemous quality ', because snails lived longer *in vacuo* than in such air, so that they died not merely through lack of common air [72]. Again, much more of a weighed perfumed cone burned in a receiver containing compressed air than in the same receiver containing air at its ordinary pressure, and, for a given quantity of air, the greater the compression, the greater the amount of the cone consumed [73]. A mouse placed in compressed air in a receiver lived longer than another mouse in a receiver of air of the same size ; and it was observed by means of a mercury gauge that ' the Mouse in the common air had consumed something of the air ' [74]. It was confirmed with another mouse ' That *living*

[68] *Experimentorum Novorum Physico-Mechanicorum Continuatio Secunda, etc.* (London, 1680).

[69] Pp. 85–94.

[70] P. 95.

[71] P. 87.

[72] Pp. 91, 95 and 97.

[73] Pp. 101–5.

[74] Pp. 74–5. With regard to the decrease of air in burning, *cf.* Hooke's experiments referred to by Lysaght in this journal (1937, **1**, 102–3). Hooke, having suggested on Nov. 13, 1672, to the Royal Society, an investigation of whether air was consumed or increased by burning, was directed to devise suitable tests, but an experiment carried out before the Society at the next meeting (Nov. 20) was unsuccessful and it was resolved to repeat it at Hooke's lodgings in Gresham College on the following Saturday before certain members of the Society and to report the result to the next meeting (Birch's *History*, iii, 61). There is no record of this result, but at the meeting of Nov. 27, ' the success not proving satisfactory ', Hooke was directed to repeat the experiment at the next meeting (*ibid.*, p. 63). At the next meeting (Dec. 4), when the experiment was called for, Hooke said that he had found neither increase nor decrease of the air, but, when the experiment was thereupon done before the Society, it failed and repetition for the next meeting was ordered (*ibid.*, p. 68) ; and it failed again on Feb. 19 following, Hooke being ' desired to fit it better for the next meeting ' (*ibid.*, p. 76). Another failure was recorded on March 5, ' the apparatus failing again ' and Hooke ' was ordered to fit it with care ' (*ibid.*, p. 77). On March 19 Hooke read a discourse ' giving an account of the success of this experiment, which, he said, he had made, about the increase or diminution of air by burning ; which was, that the air was diminished one twentieth part ', and the Society desired him to continue his experiments, to report the results from time to time and ' to bespeak some members of the Society to assist at them ' (*ibid.*, p. 78). It will be noted that neither was this important experiment successfully carried out before the Society nor have we any details of what substance was burned nor is it stated what apparatus was used.

Animals do consume air, but *dead* ones produce new '[75]; and in other experiments it was shown that a great compression of air was fatal to animal life[76]. In the experiments on combustion in compressed air, a receiver broke 'into 100 pieces', some of which wounded Boyle in the head; 'and therefore', he says, 'whilst a man looks into such Receivers, his head had need to be fortified with some perforated or pellucid muniment and defence to preserve it from a blow'[77]. In one of the experiments with animals[78], a shrew-mouse was placed in a receiver of common air, and within an hour it could hardly breathe. Boyle 'threw' another mouse into the same receiver 'and presently put on the stopple again'. But, he noted, since the first mouse had consumed some of the air, an amount of the external air forcibly entered the receiver when the stopper was taken off and dispelled 'a great part of the Air stagnant there', so that the first mouse appeared much better and lived as long as the second one, both dying in an hour. Later, he put 'a fresh strong Mouse' into the receiver 'very slowly and liesurely' to prevent the external air from expelling the included air; the mouse died in three minutes. Boyle's comment on this is: 'Whence we may conjecture, That that portion of Air which hath once served the respiration of Animals as much as it could, is no longer useful for the respiration of another Animal, at least of the same kind'[79].

Hooke's Theory of Combustion and Respiration (1665).

Meanwhile Robert Hooke had been formulating a theory of combustion and respiration, first announced in his *Micrographia* (London, 1665). Briefly, his theory was as follows: that, since wood subjected to great heat in closed vessels was charred but not consumed, while it burned away in the open air, air was the dissolvent of all combustibles, this dissolution being made 'by a substance inherent, and mixt with the Air, that is like, if not the very same, with that which is fixt in *Salt-peter*'[80]. Thus, gunpowder, since it contained salt-petre, would burn under water or in a vacuum. Similarly, in respiration, fresh supplies of air were continually necessary for the preservation of the life of animals, as it might be said, to 'blow the Fire of Life'. Hooke did not publish any work entirely devoted to this theory, but mentioned it incidentally in some of his works; and he discussed it extensively and frequently at the meetings of the Royal Society[81]. It was much neglected, but various authors continued to mention it long after Hooke's death. Volta makes a reference

[75] Pp. 80–1.
[76] Pp. 83–5.
[77] P. 105.
[78] P. 94.
[79] *Ibid.*
[80] *Micrographia*, London, p. 103.
[81] A detailed study of Hooke's theory has already been given by Lysaght in this journal (1937, **1**, 93).

to it in 1777 [82] and states that Du Hamel had quoted it in his *De Corporum Affectionibus* [83]; and Robison in 1803 [84] and Brande in 1819, in the first edition of his famous text-book of chemistry [85], referred to it after the modern theory of combustion and respiration had been established.

Mayow's 'Tractatus Quinque etc.' (1674).

John Mayow, mostly from the experiments of others, coupled however with some of his own, offered, in 1674, the same year as that in which Boyle expressed similar ideas, a common explanation for the phenomena of combustion, respiration and calcination in terms of nitro-aerial particles contained in the air [86]. He carried out experiments on combustion and respiration in limited amounts of air confined over water and noted the resultant decrease in the volume of the air and concluded that something in the air, namely, nitro-aerial particles, had been used up in these processes. To these particles he also ascribed the increase in weight observed in the calcination of antimony. Mayow's work has recently been critically examined in detail by Patterson [87], who has emphasized its many contradictions and shown that the experimental work described in it was with very few exceptions the repetition of what had already been done by others, notably by Boyle and Hooke, on these problems; a more appreciative account has since been given by Partington [88].

Newton on the respirable Part of Air (1687).

The theory that there was a vital part of the air, clearly expressed from experimental data by Boyle and Hooke and reiterated by Mayow, has a curious reflexion in Newton's *Principia* (1687). Newton, for reasons detailed, considered that the tails of comets consisted of 'a very fine vapor' [89]. He went on to say:

> 'And it is not unlikely but that the vapor, thus continually rarefied and dilated, may be at last dissipated and scattered through the whole heavens, and by little and little be attracted towards the planets by its

[82] *Lettere . . . . sull' aria infiammabile nativa delle paludi*, Milan, 1777, p. 145.

[83] *De Corporum Affectionibus cum manifestis, tum occultis, libri duo*, Paris, 1670; in *Operum Philosophicorum, etc.*, Norimbergæ, 1681, vol. ii, pp. 29–30.

[84] In his edition of Black's *Lectures on the Elements of Chemistry*, Edin., 1803, vol. i, pp. 535–7: 'I do not know a more unaccountable thing in the history of science, than the total oblivion of this theory of Dr. Hooke, so clearly expressed, and so likely to catch attention' (p. 537), following a detailed presentation of Hooke's theory.

[85] *A Manual of Chemistry*, London, 1819, pp. xxiii–xxv.

[86] *Tractatus Quinque Medico-Physici, etc.*, Oxford, 1674.

[87] *Isis*, 1931, **15**, 47 and 504.

[88] *A Short History of Chemistry*, London, 1937, pp. 80–84.

[89] *Sir Isaac Newton's Mathematical Principles of Natural Philosophy . . . . Translated into English by Andrew Motte in 1729*, edited by F. Cajori, Cambridge, 1934, p. 522.

gravity, and mixed with their atmosphere ; for as the seas are absolutely necessary to the constitution of our earth, that from them, the sun, by its heat, may exhale a sufficient quantity of vapors, which, being gathered together into clouds, may drop down in rain, for watering of the earth, and for the production and nourishment of vegetables ; or, being condensed with cold on the tops of mountains (as some philosophers with reason judge), may run down in springs and rivers ; so for the conservation of the seas, and fluids of the planets, comets seem to be required, that, from their exhalations and vapors condensed, the wastes of the planetary fluids spent upon vegetation and putrefaction, and converted into dry earth, may be continually supplied and made up ; for all vegetables entirely derive their growth from fluids, and afterwards, in great measure, are turned into dry earth by putrefaction ; and a sort of slime is always found to settle at the bottom of putrefied fluids ; and hence it is that the bulk of the solid earth is continually increased ; and the fluids, if they are not supplied from without, must be in a continual decrease, and quite fail at last. I suspect, moreover, that it is chiefly from the comets that spirit comes, which is indeed the smallest but the most subtle and useful part of our air, and so much required to sustain the life of all things with us ' [90].

This last sentence, throwing such an interesting light on the views of Newton on respiration, reads in the original :

' Porrò suspicor spiritum illum, qui aeris nostri pars minima est sed subtilissima & optima, & ad rerum omnium vitam requiritur, ex Cometis præcipue venire ' [91].

In passing, we might note here the wider extension of this idea by Newton, who says :

' The vapors which arise from the sun, the fixed stars, and the tails of the comets, may meet at last with, and fall into, the atmospheres of the planets by their gravity, and there be condensed and turned into water and humid spirits ; and from thence, by a slow heat, pass gradually into the form of salts, and sulphurs, and tinctures, and mud, and clay, and sand, and stones, and coral, and other terrestrial substances ' [92].

It is worth noting here that Pemberton, in his popular exposition of Newton's work [93], refers to these speculations about the uses of the tails of comets, speculations which, he says, he ' cannot but extreamly admire ' ; and he adds :

' It is farther our great author's opinion, that the most subtile and active parts of our air, upon which the life of things chiefly depends, is derived to us, and supplied by the comets. So far are they from portending any hurt or mischief to us, which the natural fears of men are so apt to suggest from the appearance of any thing uncommon and astonishing ' [94].

[90] *Ibid.*, pp. 529–30.
[91] *Principia*, 1687, lib. III, p. 506.
[92] Cajori's edition cited above, p. 542.
[93] *A View of Sir Isaac Newton's Philosophy*, London, 1728, pp. 243–5.
[94] *Ibid.*, p. 245.

But most of this work failed to make its proper impression, perhaps because of the explanatory success of the phlogiston theory in the early part of the eighteenth century at the hands of Stahl, bent on ordering and systematizing chemistry according to the principles of his master, Becher, possibly also because the chemistry of the air was still unknown and unexplored. Gradually, as pneumatic chemistry developed and brought with it a recognition of the existence of gases chemically distinct from air, the solution of these problems slowly became possible. At this time many other experiments, details of which have already been given by Patterson [95] and by Lysaght [96], were, however, carried out on combustion and respiration. But it long remained unrecognized that there were gases distinct from air : and while, as Partington has pointed out [97], van Helmont in his posthumous *Ortus Medicinæ* (Amsterdam, 1648) had indicated the existence of various gases, inventing the term ' gas ', he had not devised any means of collecting them. Possibly under the influence of Boyle's studies on the physical and mechanical properties of air, its chemistry was overshadowed. We find Hales in his *Vegetable Staticks* (London, 1727) collecting ' air ' from many different substances and, after measuring the yield, regarding his product as nothing other than ' air ', the cement of particulate bodies. His apparatus, however, with its pneumatic trough, marked a great practical step forward, while Brownrigg [98] collected gases from mines and the ' mineral elastic spirit ' from Spa waters, sending accounts of his work to the Royal Society for the information of the Fellows, but modestly declining to publish most of it.

The first to differentiate one gas from another effectively, or in the words of the time, one ' air ' from another, was Joseph Black. In his inaugural dissertation, *De Humore Acido a Cibis orto, et Magnesia Alba*, presented for the degree of M.D. in the University of Edinburgh on June 11, 1754, Black gave some account of his studies of *magnesia alba* ; and in an extension of this work under the title, *Experiments upon Magnesia Alba, Quicklime, and some other Alcaline Substances*, read before a society in Edinburgh on June 5, 1755, and subsequently published in 1756 [99], Black, after showing that the mild and caustic alkalis owed their chemical differences to the presence or absence of ' fixed air ' in their composition, argued that this air was ' different from common elastic air ' [100] and ' one particular species only, which is dispersed thro' the atmosphere, either in the shape of an exceedingly subtile powder,

[95] *Loc. cit.*

[96] *Loc. cit.*

[97] *Annals of Science*, 1936, **1**, 359.

[98] *Phil. Trans.*, 1765, **55**, 218 : communicated to the Society many years earlier.

[99] Published in 1756 in *Essays and Observations, Physical and Literary, read before a Society in Edinburgh* (Edin., 1756, ii, 157).

[100] *Ibid.*, p. 198.

or more probably in that of an elastic fluid ' [101]. In later work, unpublished but announced in his lectures, carried out in 1756, he showed that ' fixed air ' asphyxiated animals and that the ' air ' produced in respiration, vinous fermentation and the burning of charcoal was identical with the ' fixed air ' of the mild alkalis, since the ' air ' from each of these processes precipitated lime-water ; he took a U-tube containing lime-water and showed that precipitation occurred when he breathed out through the lime-water, while ' air ' from burning charcoal gave the same result [102]. As for the vinous fermentation, he says :

> ' In the same year [1756] I found that fixed air is the chief part of the elastic matter which is formed in liquids in the vinous fermentation . . . . I convinced myself of the fact by going to a brew-house with two phials, one filled with distilled water, and the other with lime-water. I emptied the first into a vat of wort fermenting briskly, holding the mouth of the phial close to the surface of the wort. I then poured some of the lime-water into it, shut it with my finger, and shook it. The lime-water became turbid immediately ' [103].

Some years later it is recorded that he carried out another and very different experiment, described as follows :

> ' In the winter of 1764–5, Dr. Black rendered a considerable quantity of caustic fossil alkali mild and crystalline, by causing it to filtre slowly by rags, in an apparatus which was placed above one of the spiracles in the cieling of a church, in which a congregation of more than 1500 persons had continued near ten hours ' [104].

By these researches and experiments Black had taken the first effective step in the chemistry of air and the gases and towards the solution of the problems of combustion and respiration : air contained a chemically identifiable constituent, ' fixed air ', which was different from ordinary air and which combined with the caustic alkalis, and ' fixed air ' was produced in the processes of respiration, vinous fermentation and the combustion of charcoal.

[101] *Ibid.*, pp. 198–9.

[102] Black's *Lectures on the Elements of Chemistry*, ed. J. Robison, Edin., 1803, vol. i, pp. 87–8.

[103] *Ibid.*

[104] *Ibid.*, p. 87, editorial footnote.

PARACELSUS.

A résumé of some controversies.

By A. F. Titley, B.Sc., D.Phil.

[Plates VII & VIII.]

The name of Paracelsus is of course familiar to any student of science or medicine and, up to comparatively recently, it would probably be true to say that it was familiar to any well-educated man [1]. Even to-day most people coming under the latter definition will confess to a vague recognition of the name, without usually being able to attach to it any specific associations, though they may be totally unacquainted with the names of such important contemporaries of Paracelsus as Vesalius, Paré, Georgius Agricola or Dr. John Weyer. It seems that, perhaps not unnaturally, the personality and career of Paracelsus long exercised a fascination over numerous people outside the narrow circle of specialized scholarship. Robert Browning's poem on him [2] is of course well known but, in addition to this, he has been made the subject of several plays and historical romances in different languages, and has even contributed an adjective to the English speech.

The reason for this interest seems to lie in the fact that for over three hundred years Paracelsus has been the centre of frequently recurring controversies. Dissension was naturally at its fiercest during the century or so after his death while the struggle was in progress, especially in the field of medicine, between authority on the one hand and observation and innovation on the other. Even when this battle was over, no real agreement was reached about Paracelsus himself and, though the debates tended now to be confined to academic circles, echoes from them reached the larger world and kept interest in his enigmatic

[1] For instance, the Rev. W. Tuckwell in his *Reminiscences of Oxford* (London, 1900) quotes the following epigram circulated in the House of Commons by Thorold Rogers in about 1840:—

> 'Attorney Proclus, so they say,
> Swallowed ten drachmas t'other day.
> He choked, he gasped; to ease his ill
> Came Paracelse with purge and pill.
> Seven coins the emetic spew obeyed.
> Cries Proclus, "Curse your plundering trade!
> Of my loved store three fourths are gone:
> So help you Plutus leave me one!"'

[2] *Paracelsus* (London, 1835).

character alive. There have been controversies about the manner of his life, about the circumstance of his death, about the genuineness of his works, about their originality and about their value. As a result the literature concerning Paracelsus is perhaps the largest relating to any figure in the history of chemistry between Roger Bacon and Boyle, and may reasonably be said to be somewhat out of proportion to his achievements and importance [3].

In the following paper no new facts are put forward, nor is any comprehensive attempt made to survey the life and achievements of Paracelsus. Numerous full-length biographies and critical studies, not all of equal value, have been published within the last fifty years, several of them in English [4], while the leading authenticated facts are readily accesssible in Ferguson's article in the *Encyclopædia Britannica* [5] or in E. J. Holmyard's brief chapter in his *Makers of Chemistry* [6].

In view, however, of the extremely large mass of available literature and its very varied reliability, some interest may be found in a summary of the progress of Paracelsan scholarship and the biographical and bibliographical controversies to which it has given rise. This will be prefaced by a short outline of what is generally accepted about his life, with particular attention to its bearing on the authenticity of his works.

One advantage in the study of Paracelsus is that we are dealing with a perfectly solid historical figure, about whom, in spite of some mysterious gaps, a great deal definite is known. It is not like the case of Basil Valentine or Isaac Hollandus, who, it is more than possible, never existed at all except in the imagination of their editors. There are three main sources of our knowledge about the life of Paracelsus. First there are the accounts and reminiscences of him which appeared during the century or so after his death. These are all highly polemical and must be viewed with the greatest caution ; they are

[3] For a useful bibliography of early works and references bearing on Paracelsus, see J. Ferguson, *Bibliotheca Chemica*, Glasgow, 1906, vol. ii, pp. 171–4, which lists over eighty. For nineteenth and twentieth century works (up to 1932) see Karl Schottenloher, *Bibliographie zur Deutschen Geschichte*, 1517–85, Leipzig, 1935, vol. ii, pp. 116–24, which lists over two hundred. The following important omissions from Schottenloher should, however, be noted :—H. Locker, *Th. Paracelsus, der Luther der Medizin*, Zurich, 1851 ; C. Daremberg, *Histoire des Sciences Médicales*, Paris, 1870 ; E. Radl, *Paracelsus, eine Skizze seines Lebens* (in *Isis*, 1913, i, 1) ; A. Franck, *Paracelse et l'alchimie au XVI^e siècle* (in Tiffereau, *L'or et la transmutation des métaux*, Paris, 1924) ; R. A. Liénard, *Paracelse, Etude Historique et Critique*, Lyon, 1932.

[4] Ferguson, *Bibliographia Paracelsica*, i–vi, Glasgow, 1877–96 ; Franz Hartmann *The Life of Paracelsus*, London, 1887 ; A. M. Stoddart, *The Life of Paracelsus Theophrastus von Hohenheim*, London, 1911 ; J. M. Stillman, *Paracelsus*, Chicago & London, 1920.

[5] Ninth edition (1885) and later editions.

[6] Oxford, 1931.

as a rule the products either of enthusiastic disciples or of infuriated opponents and hence contain much exaggeration [7].

Then there are contemporary documents, such as municipal or judicial records, letters written from, to or about Paracelsus during his lifetime, references to him in contemporary diaries and so on. Most of this material can reasonably be accepted as reliable; it has been very carefully collected and examined, especially by a group of German scholars at the end of the nineteenth and the beginning of the present century, the most important of whom is Karl Sudhoff, to whose monumental bibliography of Paracelsus reference will later be made [8].

Finally there are the statements that Paracelsus makes about himself in those of his works which, for various reasons, it may be assumed are authentic. These autobiographical details occur especially in prefaces or forewords to editions of the medical works. They cannot perhaps be regarded as completely reliable in view of his fondness for hyperbole, but they cannot be disregarded. They have been carefully collated by R. J. Hartmann [9] and others, and have thrown a good deal of light on some periods of Paracelsus's life hitherto obscure. From these various sources the following seem to be the facts which are generally agreed :—

[7] The following is a selection from the principal sources :—Leo Suavius, *Theophrasti Paracelsi Philosophiæ et Medicinæ utriusque Compendium*, Paris, 1567, Basiliæ, 1568; P. Severinus, *Epistola*, Basiliæ, 1572; Thomas Erastus, *Disputationum de medicina nova Philippi Paracelsi—pars prima*; *ibid.—pars altera*; *ibid.—pars tertia* (2 parts); *ibid.—pars quarta et ultima*, Basiliæ, 1572/3; Gerhard Dorn, *De naturæ Luce physica ex Genesi desumpta, juxta sententiam Theophrasti Paracelsi . . . Tractatus. Cui annexa est . . . admonitio ad T. Erastum de retractandis calumniis in Paracelsum*, Francofurti, 1583; Andreas Brentzius, *Farrago Philosophorum*, 1606; D. Sennertus, *De Chymicorum cum Aristotelicis et Galenicis consensu ac dissensu liber*, Wittebergæ, 1619: contains Oporinus's *Epistola de Paracelso ad Wierum et Solenandrum*, p. 66 f.; Conring, *De Hermetica Ægyptiorum vetere et Paracelsicorum nova medicina. Liber unus*, Helmestadii, 1648; E. Franciscus, *Die lustige Schau-Bühne von allerhand Curiositäten*, Nürnberg, 1663.

[8] The following is a list of the most important and reliable treatises bearing on the life of Paracelsus :—M. B. Lessing, *Paracelsus, sein Leben und Denken*, Berlin, 1839; F. Mook, *Theophrastus Paracelsus, eine kritische Studie*, Würzburg, 1876; J. Ferguson, *op. cit.*; E. Schubert u. K. Sudhoff, *Paracelsusforschungen*, 2 pts., Frankfurt, 1887–9; Carl Aberle, *Grabdenkmal, Schädel und Abbildungen des Theophrastus Paracelsus*, Salzburg, 1891; G. W. A. Kahlbaum, *Theophrastus Paracelsus*, Basle, 1894; K. Sudhoff, *Versuch einer Kritik der Echtheit der Paracelsischen Schriften*.—Part I. *Bibliographia Paracelsica*, Berlin, 1894: Part II. *Paracelsushandschriften*, Berlin, 1899; R. Netzhammer, *Theophrastus Paracelsus. Das Wissenwerteste über dessen Leben, Lehre und Schriften*, Einsiedeln, 1901; F. Strunz, *Theophrastus Paracelsus, sein Leben und seine Persönlichkeit*, Leipzig, 1903; H. Magnus, *Abhandlungen zur Geschichte der Medizin*.—Vol. XVI. *Paracelsus, der Ueberarzt*, Breslau, 1906; J. M. Stillman, *op. cit.*; E. Darmstaedter, *Studien der Geschichte der Medizin*.—Hft. XX. *Arznei und Alchemie. Paracelsus Studien*, Leipzig, 1931; R. A. Liénard, *op. cit.*; C. Sudhoff, *Paracelsus. Ein deutsches Lebensbild aus den Tagen der Renaissance*, Leipzig, 1936.

[9] R. J. Hartmann, *Theophrastus von Hohenheim*, Berlin, 1904.

Paracelsus, whose baptismal names were Philippus Theophrastus Bombastus von Hohenheim, was born towards the close of 1493 at Einsiedeln near Zurich—at that time part of Germany. His father was a certain Wilhelm von Hohenheim, a licentiate of medicine and apparently the local physician. As far as is known he was an only child, and his mother died in childbirth or soon after. About 1502, Wilhelm removed to Villach, a small town in southern Austria, quite near the Italian border and about 40 miles north of Caporetto, where he practised as a physician till his death in 1534. He seems to have been much respected, and there are various laudatory references to him in local archives. His portrait is in the Carolino-Augusteum Museum at Salzburg.

The boyhood and youth of Paracelsus, in fact his whole life up to the time he appears at Strassburg in 1526, remains, however, very obscure. He seems to have been instructed in medicine and some alchemy by his father, for whom he retained a marked respect and affection throughout his life. There is also little doubt that he visited and worked in the mines and metallurgical workshops of Sigismund Füger at Schwatz in the Tyrol. This was probably in about 1514, when Paracelsus was twenty-one or twenty-two. The effects of this experience are quite obvious in certain of his writings, and it is possible that his fondness for metallic remedies has the same origin.

The majority of Paracelsus's biographers mention two supposed episodes in his early life. One is a period of study at the University of Basel at various dates, ranging between 1506 and 1512 acccording to different authors. It is quite possible, in view of the fact that Paracelsus undoubtedly travelled widely, that he was at this university as a student, but there is no positive evidence. The second common story is that he was a pupil and disciple of John Heidenberg, or Trithemius as he was usually called, a theologian and neo-Platonic scholar of some celebrity towards the end of the fifteenth century. Trithemius was abbot of Spanheim from 1483–1506 and subsequently of Würzburg from 1506 till his death in 1519; he was known as a student of the Kabbalah and had a reputation as an adept in magic and alchemy. Judging from his writings, however, his attitude towards the latter was hardly that of an enthusiast and more than once he refers contemptuously to it. Ferguson, for instance, quotes a passage in which the alchemical profession is summed up as a kind of Rake's Progress:

'Vanitas, fraus, dolus, sophisticatio, cupiditas, falsitas, mendacium, stultitia, paupertas, desperatio, fuga, præscriptio et mendacitas, pedisse quæ sunt Chymiæ'[10].

The tradition that Paracelsus studied under Trithemius seems to be mainly

[10] Ferguson, *Bibliotheca Chemica*, ii, 472. I have been unable to trace the source of this quotation, but compare Trithemius, *Annalium Hirsaugiensium*, St. Gall, 1690, vol. ii, 225, 288, where the same sentiments are expressed in similar language.

founded on the slender evidence of a passage in the preface to one of his medical works, published a considerable time after his death [11]:

'From my youth I have learnt from those teachers who were most thoroughly grounded in the knowledge and arts of philosophy. First from my father, Wilhelmus von Hohenheim, who has never ceased to help me. Afterwards from a large number not necessary to enumerate besides from the writings of many others ancient and modern who have laboured greatly, as Bishop Scheyt of Stettgach, Bishop Erhart and his predecessors of Lavantall, Bishop Nicolaus of Yppon, Bishop Matthäus Schacht, suffragan bishop of Phrysingen. And many abbots, as of Spanheim and others, and many among the doctors and their like. And I have also had great experience for a long time with many alchemists who have investigated these arts, as namely with the noble Sigismund Füger of Schwatz and a number of his employed artisans.'

There is no reason to doubt the authenticity of this passage: it conflicts at no point with what has been gleaned about Paracelsus's life from other sources. Further, a spurious passage would probably have attributed the learning of Paracelsus to more distinguished teachers than this rather miscellaneous set of names, most of whom must have passed into oblivion by the time the book appeared more than half a century after his death. In the passage quoted, however, Paracelsus appears to make no direct claim to having been an actual pupil of Trithemius but merely to have read his works [12]. It is possibly significant that he refers to him as abbot of Spanheim. Trithemius left Spanheim in 1506, when Paracelsus was barely thirteen years of age, and, if the latter had ever studied under him, it would presumably have been while he was abbot of Würzburg from 1506 until his death in 1519. In such a case it seems at least likely that Paracelsus would have referred to him as abbot of Würzburg. Trithemius's *literary* reputation, however, was mainly established while at Spanheim, and there is no incongruity in Paracelsus referring to him under that name from a mere book acquaintance.

However, this story of Paracelsus studying under Trithemius and absorbing from him the neo-Platonic doctrine is almost universally repeated by his biographers, the latest of whom [13], indeed, devotes some ten pages of rhetorical speculation to the relationship of the mature scholar and the young neophyte [14].

There is little doubt that Paracelsus travelled fairly widely in early manhood, and R. J. Hartmann, a very reliable biographer, considers that he probably visited France, Belgium, England and possibly Scandinavia, though probably

[11] *Chirurgische Bücher und Schrifften*, Strassburg, 1605, pp. 101–2.

[12] Compare R. J. Hartmann, *op. cit.*, p. 17.

[13] R. Allendy, *Paracelse, le Médecin Maudit*, Paris, 1937, pp. 25–34.

[14] Sudhoff has recently put forward the view that the 'abbot of Spanheim' referred to by Paracelsus was not in any case Trithemius but a certain Bruno von Spanheim, abbot of St. Paul in Lavantall: see *Paracelsus. Ein deutsches Lebensbild etc.*, Leipzig, 1936, p. 13 f.

not Russia and the Near East as has been frequently asserted. During much of this period it is suggested that he was acting as an army surgeon in various campaigns, including the Venetian wars of 1521–1525, when Swiss mercenaries were largely used. At some time during this period he was supposed to have acquired his degree of Doctor of Medicine. Until recently there was no evidence as to when or where this degree was obtained, and Paracelsus himself is curiously secretive about it. His professional rivals during his lifetime often disputed his title to it and accused him of charlatanry, a charge to which he alludes with contempt in his writings, but to which he makes no formal reply. On the other hand he is described as a Doctor of Medicine in the records of his admission to citizen rights and to the Guild of Luzerne, to which surgeons belonged, at Strassburg in 1526 [15]. It seems hardly likely that these bodies, and a year later the Stadtartzt of Basel, would have taken his qualifications on trust, though it is, of course, possible. A document has, however, been brought to light in the municipal archives of Basel in which Paracelsus, acting as witness in a lawsuit, states on oath that he had become a Doctor of the University of Ferrara [16].

It is in the year 1526 that Paracelsus fully emerges into the light of history. At that date he had recently settled as a physician in Strassburg and, as just mentioned, taken out citizenship rights in that town. At this time Frobenius, the celebrated publisher of Basel, who had suffered for some time from a malady which the local faculty could not relieve, heard of the new physician at Strassburg and sent for him. Paracelsus effected a rapid cure, and subsequently seems to have become fairly intimate with Frobenius, with Erasmus, who was at that time living in Frobenius's house, and with Oecolampadius, the professor of theology in the University. There is a very friendly and complimentary letter in existence, written by Erasmus to Paracelsus probably towards the end of this year [17]. A short time later the three friends recommended Paracelsus to the Stadtrath of Basel for appointment to the post of Stadtarzt and Professor of Medicine which was then vacant, a post which Paracelsus promptly accepted.

The appointment lasted barely two years. Paracelsus was soon embroiled with the medical faculty owing to his repudiation, in lectures and debate, of the doctrines of Galen and Avicenna. In fact he is reputed to have enlivened his inaugural lectures by burning publicly the works of these authors. He also created a good deal of scandal and opposition by lecturing in the German vernacular instead of in Latin. Opposition grew, his lectures were interrupted, efforts were made to get rid of him by invoking obsolete University statutes, lampoons and satirical pamphlets appeared and were posted on church doors and in public places. On two occasions he appealed to the Stadtrath to uphold

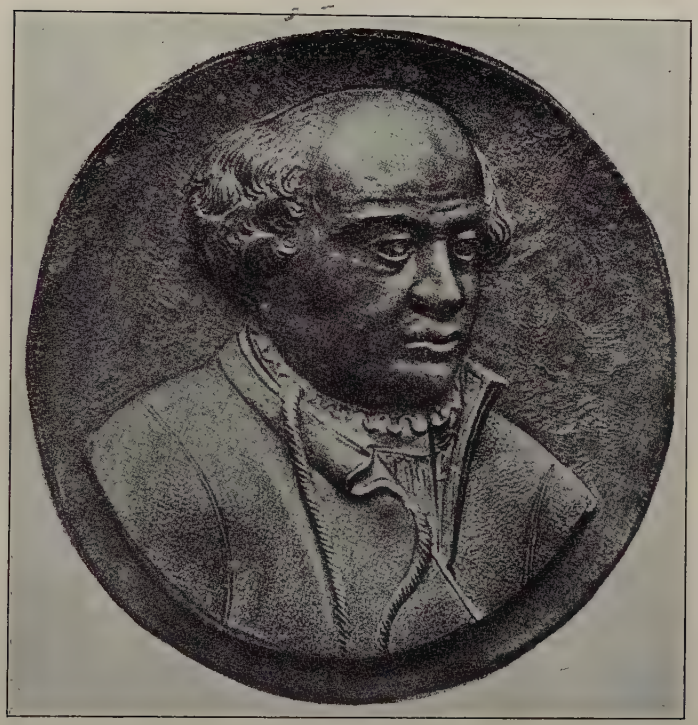
[15] Stillman, *op. cit.*, p. 15 f.
[16] A. Burckhardt, *Correspondenz-Blatt für Schweizer Aerzte*, 1914, **44**, 356, 884.
[17] Schubert u. Sudhoff, *op. cit.*, 99, 105.

his authority and to punish his attackers and, on the whole, he seems to have been supported by it, if somewhat lukewarmly. The end came, however, rather suddenly after about two years of this struggle. Paracelsus became involved in a lawsuit against one of his patients, Canon Lichtenfels, for non-payment of fees. What little we know of the case suggests that he had some justice on his side, but the judgement went against him and he lost the case. In his anger he committed what amounted to gross contempt of court and laid himself open to severe penalties. Since his numerous enemies would certainly have pressed the charge with gusto, he was advised by his friends to leave the town, which he did hurriedly, never to return [18].

When Paracelsus left Basel in 1528 he was 35 years of age and for the remaining thirteen years of his life he seems, from the evidence of letters, records and contemporary documents, to have journeyed almost continuously about Germany and Austria. From Basel he went to Colmar, thence to Esslingen and thence to Nuremberg, where we get a vivid account of him in 1529 in a book by a contemporary writer [19]. For some time he seems to have made the town of St. Gallen his headquarters, for the preface to the third edition of the *Paramirum* is dated in St. Gallen in 1531, while in the diary of a certain Johann Rutiner, a citizen of that town, for 1534–5, there is a reference to Paracelsus: 'Theophrastus is most laborious, sleeps little; without undressing throws himself booted and spurred on the bed and ceaselessly, ceaselessly writes' [20]. This glimpse is of some importance in view of the doubt that has been cast on the genuineness of Paracelsus's writings on the ground of their copiousness alone. He does not, however, seem to have been at all continuously at St. Gallen between 1531 and 1535; there is much evidence that he was travelling almost continuously, and also that during this period his fortunes were at a very low ebb. In 1534 he was refused admission to the town of Innsbruck and remarks in the preface to his treatise on *The Pestilence in the City of Sterzingen* that 'the burgomaster of Innsbruck has probably seen doctors in silks at the courts of princes but not sweltering in rags in the sun' [21].

During all this time and the succeeding years he appears to have been engaged in a long and usually unsuccessful struggle to get licences for the publication of his works in the face of stubborn opposition from the various medical faculties. In 1537 he revisited Villach where his father had died three years previously during his absence. Throughout the next few years he seems to have been rather more prosperous, and his reputation must still have been high, for there are records of his having been entertained by

[18] Christian Wurstisen, *Bassler Chronick*, 1580, p. 555 f.
[19] Sebastian Franck, *Chronica, Zeytbuch und Geschichtsbibel*, Ulm, 1536, i, 279.
[20] Quoted by Stillman, *op. cit.*, p. 164. See also Schubert u. Sudhoff, *op. cit.*, i, 63.
[21] *Huser Collection*, 1589, iii, 85.



Portrait medallion of Paracelsus by Georg Schweigger.
(Carolino-Augusteum Museum, Salzburg.)

prominent citizens [22]. Finally in April 1541 he arrived at Salzburg, where he was received by the Archbishop Ernst and where he remained until his death in September of the same year. He was buried in the churchyard of St. Sebastian.

Much ink has been spilt over the circumstances of his death. The antagonistic chroniclers of the end of the sixteenth century attributed it to a drunken orgy, his defenders to assassination by hirelings of his medical opponents. The remains have been exhumed on several occasions; firstly about fifty years after his death during certain additions to the church; then in 1752 by the Archbishop Andreas von Dietrichstein, who removed them to a marble tomb beneath the church porch. At the beginning of the nineteenth century the dispute about his death was revived, when a certain Dr. von Sommering obtained permission to examine the remains and claimed to have found a fracture of the skull which could only have been the result of a blow or a fall [23]. The question was re-examined by Aberle between the years 1878 and 1886, and in 1891 he published a large work on this subject and on the existing portraits and memorials of Paracelsus. Aberle decided that the fracture was really a fissure of rachitic origin probably enlarged by careless treatment of the bones during the earlier exhumations [24]. Since, however, it is known that Paracelsus's will, the attested original of which has been preserved, was drawn up three days before his death, it seems certain that the death was not sudden and unexpected and that, therefore, Aberle's view is probably correct [25].

It is not proposed here to enter into a lengthy discussion of Paracelsus's personality, about which again opinion has differed violently. It is sufficient to say that he was undoubtedly a man of an extremely arrogant nature and violent temper—the internal evidence in his writings is enough to prove that—but on the whole modern estimates have tended to discount the view often put forward by his critics that he was an intemperate charlatan or a mere superstitious mystic. The charge of habitual drunkenness goes back to the violent attacks of Erastus [26], who never knew Paracelsus personally and who relied on the evidence of Oporinus [27], now recognized as a rather unsatisfactory witness. As for the charges of charlatanry and ignorance, it is true that the extreme obscurity and eccentricity of Paracelsus's style lends some colour to the former, though the views actually expressed are consistent enough among themselves and with the particular contemporary philosophic outlook which

[22] F. Strunz, *op. cit.*, 73.

[23] J. N. Ehrhart, *Medicinisch-chirurgische Zeitung*, 1815, **1**, 47.

[24] C. Aberle, *op. cit.*, 57 f.

[25] For a review of the evidence see Sudhoff, *Deutsche Med. Wochenschrift*, 1891, **17**, 1114–5.

[26] *Op. cit.*, *passim*.

[27] Sennertus, *op. cit.*, 66.

he had adopted. His writings do not suggest that he was in any sense either a profound scholar or a particularly gifted observer, but they do show a considerable acquaintance with the thinkers of his own time and especially with the neo-Platonists of the fifteenth-century Florentine school. How far he can be regarded as an original observer is a question which will be referred to briefly later.

Coming now to a consideration of his writings: these may for convenience be divided into (1) those published during his lifetime; (2) those published after his death but prior to the year 1590; (3) those published after 1590.

If we are to include the brief advertisement and programme of his lectures at Basel, published in 1526, then a total of thirteen works are known to have been published in his lifetime [28], of which Sudhoff enumerates a total of twenty-three editions between 1526 and 1540.

Although this sounds a considerable amount, the actual volume is comparatively small, several being small prophetical tracts of an astrological nature. It was of course common form in the early sixteenth century for physicians to combine astrology with medicine and to publish almanacs and prophetical pamphlets. In fact Paracelsus is only part author of one of those (No. 6) included in the total, sharing the oracle's mantle with others. The best known of these prophetical works was the *Prognosticatio* (No. 4), published in 1530 and again in 1537, and subsequently in numerous editions. French

[28] The remaining twelve are:

1. *Practica D. T. Paracelsi gemacht auff Europen anzufahen in dem nechst künfftigen Dreyssigsten Jar. Biss auff dz Vier und dreyssigst nach volgend* (Augspurg, 1529).

2. *Vom Holtz Guaiaco grundlicher heylung* (Peypus, Nürnberg, 1529).

3. *Durch den hochgelehrten Herrn Theophrastum von Hohenheim. Von der Frantzösischen Kranckheit. Drey Bücher* (Peypus, Nürnberg, 1530).

4. *Wunderbarer Geschichten so in vier Jaren nach einander,biss man zeht M.D. unnd xxxiii Jar zukunfftig Prognosticatio* (Egenhof, Strassburg, 1530).

5. *Von der Impostur der Artzney. Drey Bucher* (1530).

6. *Prophecien und Weissagungen als Doctores Paracelsi, Johann Lichtenbergers, M. J. Grunpeck, J. Carionis, Der Sibyllen und anderer durch figuren angezeygt* (1530).

7. *Die grosse Wundartznei. Drey Bucher* (Ulm & Augspurg, 1530–7).

8. *Usslegung des Commeten erschynen im hochbirg, zu mittem Augstem Anno* 1531 (1531 ?).

9. *Bad (von dem) Pfeffers in Obserschuytz gelegen* (1534).

10. *Wunderbarlicher (von den) übernatürlichen zeychen so in vier jaren ein ander nach imm hymmel gewülcke und lufft ersehen, Von Sternen Regenbögen, Fewwrigen, Platrigen, Wilde theirer Tracken schiessen, Fewrin mann mit sampt ander dergleychen. Auch Ausslegung der zweigen Commeten, so biss her yrrig aussgelegt seynd* (1534).

11. *Erklärung der Magischen Figuren welche zu Nürnberg gefunden worden* (Muhlhausen, 1536).

12. *Philosophia Sagax* (Wien, 1537).

N.B.—No. 10 does not appear in the Huser collection.

and English translations appeared as recently as 1933. The ' prophecies ' are extremely vague and nebulous, the most interesting point about the work being the curious symbolical illustrations which accompany them. These works, however, are of no importance in alchemical or medical history, nor is the *Philosophia Sagax* (No. 12), which is a cosmological treatise on the neo-Platonic lines fashionable at the period and containing an exposition of the doctrine of the macrocosm and the microcosm. The only works of this period

Symbolical illustrations from the *Prognosticatio.*

of publication that are outstanding are the three books of the *Greater Surgery* (No. 7), published between 1530 and 1537 at Ulm and Augsburg, and perhaps the treatise *On Syphilis* (No. 3), for a reason to be referred to later. It may be noted that nothing bearing directly on the theory or practice of Alchemy is included in this period.

From 1540 to 1549 there is a gap in which, according to both Ferguson and Sudhoff, no works are known to have appeared. From 1549 onwards, however,

new books appear in ever increasing numbers and multiplicity of editions. Some of these were undoubtedly authentic, some certainly spurious. There is ample evidence that Paracelsus left behind him a great deal of unpublished works in manuscript. 'Seven manuscript volumes of medical treatises' and 'various similar collections' are mentioned in his will; he is known to have sent four manuscripts to the authorities of the Archduchy of Carinthia in 1538 with the request that they should be published, a request which was ignored during his lifetime.

The *Greater Surgery* had had a pronounced and continuous success and its author's advocacy of chemical remedies, in place of the herbalistic ones prescribed by the accepted Greek and Arabian authorities, was winning eager support among many of the younger men. The movement was strenuously resisted by the Faculty and a violent struggle ensued. Curious incidents occurred. The University of Heidelberg was compelled by a student revolt to eliminate the oath pledging candidates for degrees to oppose the use of chemical remedies, and the University of Paris was forced to cancel similar legislation by opposition among students and members of the medical progressive party [29]. Paracelsus became the hero and symbol of the progressives and, naturally, there was a great demand for his writings. Supply soon appeared to meet the demand. The manuscripts were eagerly sought for and published; other works appeared which were said to have been dictated to amanuenses such as Oporinus; books were even compiled by editing the lecture notes of students during the Basle period. And finally there is little doubt that a good many people seized the opportunity presented by this boom to publish under Paracelsus's name works which were quite spurious. This seems to have been realized at a very early date, as a letter written by a certain Bartholomaus Schobinger in 1576 and quoted by Schubert and Sudhoff [30] shows:

'Theophrastus, with whom I was well acquainted, lived for twenty-seven weeks in my brother-in-law's house and deposited there many books . . . there are also many books printed under Theophrastus's name which he never knew of nor wrote. For I knew well Theophrastus's style and his habits in writing.'

That the existence of forgeries was common knowledge seems also to be confirmed by the extreme pains taken by Huser, the first editor of Paracelsus, to authenticate the origin of the works published in his collection.

Sudhoff catalogues no less than 191 editions of some seventy to eighty works purporting to be by Paracelsus which appeared between 1549 and 1589, while by 1658 the total editions had reached nearly four hundred.

The date 1589 is of great importance in the history of Paracelsan literature, for it was then that there began to appear the first collected edition of his works,

[29] Stillman, *op. cit.*, 168.
[30] *Paracelsusforschungen*, ii, 142.

Erſter Theil
Der Bücher vnd Schrifften / des
Edlen / Hochgelehrten
vnd Bewehrten PHILOSOPHI
vnd MEDICI,
PHILIPPI THEO-
PHRASTI Bombaſt von Ho-
henheim / PARACELSI
genannt:

Jetzt auffs new auß den Originalien / vnd Theo-
phraſti eigner Handſchrifft / ſoviel derſelben zube-
kommen geweſen / auffs trewlichſt vnd
fleiſſigſt an tag geben:

Durch
IOHANNEM HVSERVM BRISGOIVM
Churfürſtlichen Cölniſchen Rhat vnnd
MEDICVM.

In dieſem Theil werden begriffen die Bücher / welche von
Vrſprung vnd herkommen / aller Kranckheiten handeln in
GENERE: Deren Catalogus nach der Præ-
fation an den Leſer zů finden.

*Adiunctus eſt INDEX Rerum & Verborum accu-
ratiſſ. & copioſiſſimus.*

Getruckt zu Baſel / durch
Conrad Waldkirch.
ANNO M. D. LXXXIX.

Title-page of first collected edition of Paracelsus's works by Huser (1589).

which is usually known as the ' Huser Collection ' [31]. It was completed in 1591 and ran to ten volumes, containing a total of ninety-nine different works of very varying length, some being mere pamphlets of a few pages and some quite considerable treatises. It would have been in any case interesting to have a collection which showed what was commonly accepted as the ' Paracelsan canon ' some fifty years after his death, but it also very fortunately happened that the compiler, Johannes Huser, appears to have been for his date a very scrupulous and careful editor. He scoured Germany and Austria for manuscripts and early editions and prefixed to each work a brief account of the source, stating when it is printed from a manuscript in Paracelsus's own hand, when from a properly certified copy and when, in his opinion, the authenticity is doubtful. Huser included several works of the latter class, and it may be noted here that he questions the genuineness of all the books which deal *exclusively* with Alchemy. Notwithstanding this there is a good deal of Alchemy in works of Paracelsus that are said by Huser to be genuine, but it is scattered throughout the medical works and general philosophical writings.

At the commencement of the seventeenth century, soon after the appearance of the Huser edition, began the Basil Valentine controversy. A certain Johann Thölde in the year 1600 began to publish the works of an alleged Benedictine monk called Basilius Valentinus, whose Latin manuscripts of the early fifteenth century he claimed to have discovered and to have translated into German. These books, especially the *Triumphal Chariot of Antimony*, attracted much attention, for they contained some important chemical facts, and it was soon observed that they duplicated to a remarkable extent the information and the style of presentation of Paracelsus. Both attacked the traditional medicine, both advocated chemical remedies, especially preparations of antimony, and both used the doctrine of the ' tria prima ' or ' hypostatical principles '—Sulphur, Mercury and Salt—which had hitherto been supposed to have been originated by Paracelsus. Actual chemical operations were often more clearly and accurately described by ' Basil Valentine ' than by Paracelsus, but the general outlook of both authors was extremely similar. These publications appeared just at the time when the struggle between the supporters of Paracelsus and his opponents was at its height and the latter, of course, seized the obvious opportunity of accusing him of plagiarism. It was claimed that Paracelsus had somehow obtained access to the Valentine manuscripts, concealed them, and used the knowledge thus illicitly gained to bolster up his spurious reputation as an alchemist. It is true that, even at the time, certain writers expressed doubts about Basil Valentine; the rolls of the Benedictine order to which he was supposed to have belonged did not confirm

[31] *Der Bücher und Schrifften des Edlen, Hochgelehrten und Bewehrten Philosophi und Medici, Philippi Theophrasti Bombast von Hohenheim, etc.*, 1st edn., Basle, 10 vols. quarto (1589–91); 2nd and 3rd edns., Strassburg, folio (1603 and 1616).

the account of him given by Thölde, and it was thought odd that no one had ever mentioned or quoted him previously. As time went on these doubts grew. It was noticed that Valentine used terms like ' the French Evil ' for syphilis [32], which were not current in Germany before 1500, while he referred to the use of antimony in typography, another practice dating from the close of the fifteenth century. It therefore became necessary to move Valentine's date forward to about 1500 and make him a contemporary of Paracelsus instead of a precursor. This, however, did not make the plagiarism an impossibility though it rendered it far more improbable. The actual existence of Basil Valentine remained, nevertheless, fairly generally accepted until the end of last century, when the question was thoroughly reinvestigated by Hermann Kopp, who eventually arrived at the conclusion that all the Valentine literature was a forgery or series of forgeries of the end of the sixteenth century, and that in all probability Thölde was himself the author of them [33]. This opinion has been confirmed by Sudhoff [34] and others, who take the view that the plagiarism was in the reverse direction and that Thölde compiled his forgeries after a study of Paracelsus, Agricola and perhaps one or two later alchemists. Little doubt therefore remains that Paracelsus is exempted from the charge of plagiarism, though the precise origin of the Valentine writings is still somewhat obscure, and he is frequently still treated as a genuine historical figure by historians of chemistry.

During the seventeenth century the publication of Paracelsus's works steadily continued, though on the whole it was mainly new editions of books which had already been collected by Huser which appeared. There were a few fresh ones and several apocryphal alchemical works but nothing of much importance. Numerous translations were current and a catalogue of those in English has been made by Ferguson [35]. There were also one or two fresh collected editions, the last important one being the folio Latin version, entitled *Opera omnia medico-chemico-chirurgica*, issued at Geneva in 1658.

What we may call the modern period of Paracelsan scholarship was inaugurated in 1876, when Friedrich Mook's *Theophrastus Paracelsus, eine kritische Studie* appeared. This was not in fact so much a critical study of Paracelsus as a bibliography of his writings and a discussion of their authenticity. Mook had collated Huser's edition with editions of Paracelsus which had appeared during the latter's lifetime and found the correspondence to be so exact that he suggested Huser's statements on the sources of the different

[32] See footnote [28], 3.

[33] Kopp, *Die Alchemie in älterer und neuerer Zeit*, Heidelberg, 1886; *Beiträge zur Geschichte der Chemie*, Braunschweig, 1869–75.

[34] Stillman, *op. cit.*, p. 97, quotes a private communication from Sudhoff in which the latter states that, after looking through many thousands of mediæval manuscripts, he has absolutely no doubt that nothing like Basil Valentine existed previous to Hohenheim.

[35] *Bibliographia Paracelsica*, part 3.

works might also be safely regarded as reliable. This criterion was rather vigorously criticized by Ferguson in the following year [36], who, however, seems to have been somewhat prejudiced against Mook by finding minor inaccuracies in his bibliographical work. Mook's general contention is fairly widely accepted, and most later scholars have regarded the evidence provided by Huser as fundamental. Ferguson subsequently published several more studies of Paracelsan bibliography, including a most useful alphabetical list of all the editions of Paracelsus's works known to him [37].

The most comprehensive and authoritative work in this field, however, was undoubtedly that of Sudhoff, who devoted a large part of his life to the subject and travelled all over Europe, examining and collecting editions, letters and information concerning Paracelsus. After some preliminary studies published in collaboration with Schubert, the final fruits of his encyclopædic knowledge appeared in the two volumes of his *Versuch einer Kritik der Paracelsischen Schriften*. The first volume, issued in 1894, is a bibliography of the published works, while the second volume, issued in 1899, is a study of the known letters and manuscripts. This work is indispensable to anyone studying Paracelsus, and is unlikely to be superseded.

This account of the literature may be concluded by a reference to the question of modern translations of Paracelsus. One of the difficulties in studying the works is the odd mixture of old German, dog-Latin and a sort of private vocabulary which he employed in his writings. That this difficulty was a very real one even to his contemporaries is witnessed by the appearance, quite soon after his death and subsequently over a long period, of numerous books designed to elucidate the obscurity of his language [38]. In the seventeenth century a good many of the works appeared in translations or in a Latin guise—as for instance in the Latin folio of 1658. These later editions, however, are not very trustworthy as accretions, and mistranslations have tended to accumulate in them. Franz Strunz, who published in 1903 a valuable study of Paracelsus, also projected a translation of the works into modern German, but only the *Paragranum* and the two volumes of the *Paramirum* were completed by him [39]. The task, however, has been accomplished by Bernard Aschner,

[36] *Ibid.*, part 1.

[37] *Ibid.*, part 5.

[38] *Inter alia*: M. Toxites, *Onomasticum Th. Paracelsi hoc est earum vocum, quarum in scriptis ejus solet usus esse, explicatio*, Argentorati, 1574; Gerhard Dorn, *Dictionarium Th. Paracelsi continens obscuriorum vocabularum quibus in suis scriptis passim utitur*, Francofurti, 1584; M. Sendivogius, *Novum Lumen Chymicum, etc.*, Genevæ, 1628; G. Johnson, *Lexicon Chymicum, cum obscuriorum verborum et rerum hermeticarum, tum Phrasium Paracelsicarum in scriptis ejus et aliorum chymicorum explicationem continens*, Londini, 1652: republished, with a second part, in Manget, *Bibliotheca Chemica Curiosa*, i, 217 ff., Genevæ, 1702. It should be noted that the 'obscurity' was often that of spurious works.

[39] *Das Buch Paragranum*, Leipzig, 1903; *Volumen Paramirum und Opus Paramirum*, Jena, 1904.

who about six years ago completed a version in modern German of the whole Huser collection [40]. It is prefaced by an interesting historical study of Paracelsus and the literature concerning him and also reproduces some of the symbolical illustrations. A minor omission is that of Huser's editorial comments on the different works, so that it needs to be used in conjunction with Sudhoff [41].

There exists one considerable translation into English, produced by A. E. Waite in 1894 [42]. This, however, cannot be described as entirely satisfactory. Rather unfortunately Waite completed this translation immediately before the appearance of Sudhoff's bibliography, and thus had not the advantage of the critical canons laid down in that work. The source used was the Geneva Latin folio of 1658 which, as previously remarked, is not so reliable as the Huser collection. Another difficulty in using Waite's translation is that he has converted the titles of the books into English without always indicating clearly the original title, so that a few of them are difficult to identify unless access to the original text is possible. It appears that Waite's concern was more with what he terms the ' esoteric philosophy ' of the works than with their content from the point of view of the historian of science or with the question of their authenticity. In his preface he does, it is true, suggest doubts of the genuineness of three items—*The Aurora of the Philosophers*, *The Spirits of the Planets* and the *Manual of Paracelsus*, but he states that ' there is nothing else which from the internal evidence is unlikely to have been the work of Paracelsus ', a statement which can hardly be reconciled even with the editorial comments of Huser. We may say, in the light of Sudhoff's investigations and modern Paracelsan scholarship generally, that quite half of the works translated by Waite from the Latin folio, however interesting they may be as post-Renaissance alchemical writings, are not by Paracelsus. Nevertheless, for those who wish to get some idea of Paracelsus's thought in English, a good deal of this translation is very helpful, notably the version of the *De Natura Rerum* and of the *Archidoxa*, two works that are almost certainly authentic, and which contain a great deal that is typical of his outlook on Alchemy.

It is not possible in this paper to explore that outlook, a task which it is hoped may be attempted later. The main object here is to summarize the previous sources and available literature. In the meanwhile, however, this account of Paracelsus may be rounded off by a few tentative generalizations about the place which he occupies in the early history of chemistry.

[40] Bernard Aschner, *Paracelsus Sämtliche Werke*, 4 vols., Jena, 1926–30.

[41] It should be mentioned that Sudhoff is himself issuing an edition of Paracelsus's works, of which fourteen volumes have so far appeared, e. g., *Theophrast von Hohenheim, genannt Paracelsus. Sämtliche Werke.* Herausgegeben von Karl Sudhoff. 1 Abt.: *Medizinische, naturwissentschaftliche und naturphilosophische Schriften*, Otto Wilh. Barth und R. Oldenburg, München–Berlin, 1922–33.

[42] A. E. Waite, *The Hermetic and Alchemical Writings of Paracelsus*, 2 vols., London, 1894.

From a study of Paracelsus's writings it is plain that he was not an alchemist in the accepted sense of the word : his main preoccupation was with medicine. While he does not deny the possibility of transmutation, he constantly reiterates in the medical works that this is not the task of the disinterested philosopher. ' Many have said of alchemy ', he writes, in the *Fragmenta Medica* [43], ' that it is for making gold and silver. For me such is not the aim but to consider only what virtue and power may lie in medicines.' In the *Paragranum* he says similarly: ' Not, as is said, alchemy to make gold and silver : here the purpose is to make arcana and direct them against diseases ' [44].

Such constantly recurring statements, the evidence of the whole manner of his life, the judgements of Huser, and some differences in the style of the writings themselves, combine to make it certain that the numerous books of pure Alchemy (such as the *Liber Vexationum*, *De Tinctura*, *Thesaurus Alchemistorum*, *De Transmutationibus Metallorum*, *Monarchia* and several others) which have at various times been attributed to Paracelsus and which concern themselves with the ' Great Work ', are not authentic. It is noteworthy that, while Paracelsus's style is almost always obscure, he limits himself in the alchemical portions of the medical works to the use of the planetary names for the metals, *e. g.* Mars for Iron, Jupiter for Tin and so on. It is only in the supposed alchemical works that we meet with the Red Lions, Green Dragons and White Crows so dear to the hearts of the gold-seeking alchemists of the period. While it is possible that Paracelsus may have written one or two alchemical works as pot-boilers, just as he wrote astrological predictions, his whole manner of life as a wandering physician, enjoying usually a high degree of popularity, renders it almost impossible that he could have devoted any appreciable time or energy to the problem of transmutation [45]. For the same reasons, no doubt, we find when we examine the matter that his practical contributions to chemical knowledge are really rather slight. He evidently had a pretty extensive acquaintance with such chemical processes as were generally known in his time, but little that he describes cannot also be found in his contemporaries or immediate predecessors. It is noticeable that he is at his most straightforward and original whenever he is dealing with practical metallurgical operations, a fact that suggests that most of his practical experience was derived from the period spent in the Tyrolean mining area.

[43] Aschner, *op. cit.*, ii, 531.

[44] ' Nicht als die sagen/ALCHIMIA mache Gold/mache Silber : Hie ist das fürnemmen/ mach ARCANA, unnd richte dieselbigen gegen den kranckheiten.' *Cf.* Huser Collection (1589), *Das Buch Paragranum*, ii, 54.

[45] Oporinus, in his account of Paracelsus's life after leaving Basel, describes him as ' having his lodging always well supplied with charcoal for the perpetual fire (Athanor) ; sometimes heating alkalis, sometimes oil of mercury sublimate, sometimes oil of arsenic, sometimes the saffron of Mars and I know not what other concoctions. At times he pretended to prophesy and made claims to secret knowledge ' (Sennertus, *op. cit.*, 67). There is, however, no mention of transmutations.

While his descriptions of mining operations are not nearly so precise as those of his contemporary Agricola, they show considerable observation and practicality [46]. Similarly with many of the descriptions of the properties of metals and metallurgical operations. As is well known, he describes zinc for the first time, at least under that name, and he is clear on the distinction between 'vitriols', which have a metallic basis, and 'alums', which have an earthy one.

As an example of Paracelsus's plain instructions, we may take his directions for preparing the lead oxides. He says: 'If we wish to make red lead out of lead, it must first be calcined to ashes and afterwards strongly burnt laterally in a glazed jar, stirring it continually with an iron wire until it grows red. This minium is at once the best and most valuable and should be used in medicine as well as in alchemy. The other which dealers sell in the shops is of no use. It is made up only of the ashes which remain in the liquefaction of lead ore and the potters buy it for encrusting vessels. Such minium is useful only for pictures, not for medicine. In order to reduce lead to a yellow colour' (by which he means massicot) 'we must proceed in a similar way. Here too the lead must be calcined with salt and reduced to ashes. Afterwards it must be stirred continually in one of the wide dishes, used by those who test minerals, over a *moderate* coal fire, careful watch being kept lest the heat be too great or the stirring neglected. Otherwise it would melt and produce a yellow glass [47]' (by which he obviously means litharge).

On the theoretical side of Alchemy, his views are little different from those of his contemporaries, who also adopted the neo-Platonic outlook, such as Cornelius Agrippa. His most influential piece of theorizing was, of course, the well known substitution of the hypostatical principles, Sulphur, Mercury and Salt, for Geber's Sulphur and Mercury and the extension of this theory from metals to matter in general. Unfruitful as this theory was, it probably did act as a considerable stimulant to investigation, since it was possible by means of it to give at least a hazy interpretation to a much larger variety of chemical changes and phenomena.

Paracelsus is always given credit for having instilled new life and vigour into chemistry by substituting the medical aim for that of transmutation. There is, however, one way, perhaps not always stressed sufficiently, in which Paracelsus's views rather directly affected the ultimate progress of chemistry. This was that they represented an initial stage in the development of that important conception, the 'chemical individual' or the 'pure substance'. That this happened seems to have been partly inevitable, partly accidental. The inevitability is bound up with the nature of Paracelsus's medical views.

[46] In the *De Natura Rerum* considerable space is devoted to an exposition of the characteristics and working of metallic deposits, in which he ridicules some of the prevalent beliefs, such as that veins running from east to west are always the most prolific. *Cf.* Aschner, *op. cit.*, iii, 297 f.

[47] *De Natura Rerum.* Cf. Aschner, *op. cit.*, iii, 249.

Unlike the Galenists with whom he quarrelled so violently, he rejected the extremely complex mixtures of herbal remedies which they advocated in favour of what he called ' arcana ', that is, simple extracts from plants or minerals [48]. It was his view that the more the ' magistery ' of a substance, as he termed it, was freed from contaminants or unnecessary constituents, the more efficacious it would be. This doctrine was bound to focus the attention of his followers on the question of purifying their products properly, and this in its turn led to the discovery of tests and standards of purity. The accidental effect of the Paracelsan doctrine on the development of the idea of the ' pure substance ' is more curious and may not be so important. There is little doubt that, in the first flush of enthusiasm for chemical remedies and even for some time after, very reckless use was made of metallic preparations of antimony, arsenic, mercury, the vitriols and so on. No doubt a good many of the unfortunate patients were accidentally murdered ; this was one of the accusations hurled against the Paracelsans by their opponents. In any case it soon became realized that with these new remedies the exact dosage was a very important matter indeed. The effects of small quantities had to be studied, and it became plain that no consistent clinical results could be got unless the drugs were also of consistent purity. A quantitative aspect was given to the idea of purity ; Paracelsus's ' arcana ' blossomed eventually into the ' active principles ' of modern pharmacology. The path which was to lead through van Helmont, Black and Lavoisier to the laws of the conservation of matter and of constant composition may thus be faintly traced back to the teachings of Paracelsus.

[48] See E. Darmstaedter, *Paracelsus und die Einführung chemischer Präparate als Heilmittel* (in *Historische Studien und Skizzen zu Natur und Heilwissenschaft*, Festgabe G. Sticker dargeboten, Berlin, 1930).

AL-RĀZĪ AND ALCHEMY.

By Gerard Heym.

In recent years our knowledge of the chemistry [1] and alchemy [2] of the peoples living under Islam has been greatly extended by a careful study of the texts and a renewed investigation of their sources [3], and by the editing and translating of texts that had hitherto been overlooked or even unknown.

One of the greatest exponents of the scientific attitude of Islam is Abū Bakr Muḥammad ibn Zakarīyā al-Rāzī, who lived, according to al-Bīrūnī,

[1] A History of Chemistry in Mediæval Islam is being prepared by E. J. Holmyard for the ' E. J. W. Gibb Memorial ' Series.

[2] Some of the more recent publications are :—

a. J. Ruska, *Arabische Alchimisten*, vols. i & ii, Heidelberg, 1924.

b. E. J. Holmyard, *Kitāb Al-'Ilm Al-Muktasab Fī Zirā'at Adh-Dhahab*, text, translation and introduction, Paris, 1923.

c. J. Ruska, *Tabula Smaragdina*, Heidelberg, 1926.

d. E. J. Holmyard, *The Arabic Works of Jâbir ibn Hayyân*, vol. i, Paris, 1928.

e. Re-issue of the 1678 edition of Richard Russel's *The Works of Geber*. By E. J. Holmyard, with an introduction, London, 1928.

f. J. Ruska & P. Kraus, *Der Zusammenbruch der Dschābir-Legende*, Berlin, 1930 (*Jahresbericht d. Forschungsinst. f. Geschichte d. Naturwissentschaften*).

g. J. Ruska, *Turba Philosophorum*, Berlin, 1931.

h. J. Ruska, *Das Buch der Alaune und Salze*, Berlin, 1935.

i. P. Kraus, *Jābir Ibn Hayyān*, vol. i (text) ; vols. ii & iii, translation and notes, to follow, Paris, 1935.

j. J. Ruska, *Al-Rāzī's Buch Geheimnis der Geheimnisse*, Berlin, 1937.

k. E. Darmstaedter, *Die Alchimie des Geber*, Berlin, 1922.

l. For detailed bibliography see following articles in *The Encyclopædia of Islam* : vol. ii, ' Al-Kīmiyā ', by E. Wiedemann ; suppl. no. 2, ' Ismā'iliya ', by W. Ivanow ; suppl. no. 1, ' Djābir ', by P. Kraus.

[3] *a.* For a complete list of Prof. Ruska's works on this subject see *Julius Ruska und die Geschichte der Alchimie*, Verlag Dr. Emil Ebering, Berlin, 1937.

b. For articles on source material see bibliographies appended to relevant references in *the Encyclopædia of Islam* and supplements.

c. See also E. O. von Lippmann, *Entstehung und Ausbreitung der Alchimie*, vol. i, Berlin, 1919 ; vol. ii, Berlin, 1931.

d. For early sources, metals and non-metals, see J. R. Partington, *Origins and Development of Applied Chemistry*, London, 1935.

e. See especially M. Plessner, ' Neue Materialien zur Geschichte der Tabula Smaragdina ', *Islam*, vol. xvi, Berlin, 1927.

f. See also Reitzenstein & Schaeder, *Studien zum Antiken Synkretismus*, Leipzig, 1926.

from 865 A.D. to 925 A.D. [4]. He was born [5] in Rayy (Rhages), Persia, hence his name al-Rāzī—an ancient city that had already been a centre of civilization before the time of the Sassanians [6]. During the lifetime of al-Rāzī, Rayy was one of the most important cities in Islam, and it was here that he was imbued with that free-thinking spirit and criticism of established dogmas so characteristic of the Persian. He may have studied philosophy under al-Balkhī [7] and he was also interested in poetry, metaphysics, logic and music—the authorship of an encyclopædia of music is attributed to him—and he is supposed to have played the lute very well and to have been a singer [8]. At the age of thirty he went to Bagdad, and there, meeting with an old apothecary, who turned his attention to the nature of medical remedies and to unusual medical cases, he decided once and for all to devote his life to the study and practice of medicine [9]. One of his biographers [10] records a visit to the hospital in Bagdad [11], the same hospital which was later rebuilt and became famous as the 'Aẓudi' hospital, and it is thought that he was consulted about the proposed new building. Al-Rāzī's success as a physician in Bagdad was so great that he was made chief-physician-in-charge of the hospital there. Some biographers claim that he first returned to Rayy and became administrator of the hospital in that city, and then was recalled to Bagdad.

Al-Rāzī had by this time achieved very great fame as a physician. This reputation was to last, not only in the countries under Islam, but in Europe as well, throughout the Middle Ages until the early seventeenth century, when his works on medicine were still part of the curriculum at Dutch Universities. After a sojourn of some years at Bagdad, al-Rāzī returned to his native city and remained there, or, according to other biographers, lived for a time an unsettled life at the courts of princes, moving from city to city, before finally going back to his early home.

There are so many anecdotes about al-Rāzī that only one will be repeated

[4] J. Ruska, 'Al-Bīrūnī als Quelle für Das Leben und die Schriften al-Rāzī's', *Isis*, vol. v, Oct. 1923, pp. 26–50.

[5] See G. S. A. Ranking, 'The Life and Works of Razi (Rhazes)', *XVIIth International Congress of Medicine*, London, 1913. Section xxiii, pp. 237–68.

[6] Article 'Raiy' by V. Minorsky, *Encl. of Islam*, vol. iii, pp. 1105–8. See also V. Minorsky's trans. of *Ḥudūd al-'Alam*, p. 132, London, 1937.

[7] See Yākūt, *Dictionary of Learned Men* (Arabic text), ed. Margoliouth, 'Gibb Memorial Series', i, 141, Leyden, 1907–31. See also Flügel's article on the 'Fihrist', *Z.D.M.G.* xiii, 4, p. 636, Leipzig, 1859.

[8] Ranking, *op. cit.*

[9] See M. Meyerhof's articles, *Isis*, xiii (1935), pp. 100–20; *Isis*, xxiii (1935), pp. 321–72; *Mitteilungen z. Gesch. d. Med. u. Naturwiss.* xxix, 278 (1930).

[10] See Ranking, *op. cit.* For a more detailed life of al-Rāzī based on a greater knowledge of the sources see the excellent article by P. Kraus and S. Pines, 'al-Rāzī', *Encl. of Islam*, vol. iii, pp. 1134–6.

[11] For the reputation of the hospital in Bagdad see *Arabian Nights*, Benares ed., trans. by R. F. Burton, vol. ii, p. 70.

here [12]. Asked by a stranger about al-Rāzī, a native of Rayy gave the following description of the great man. Al-Rāzī, an old man now, was surrounded by his pupils in the lecture hall, and behind these sat their pupils and behind these there were more pupils of pupils. Al-Rāzī had a large stomach and running eyes, the latter having been caused by an over-indulgence in eating garlic (or beans) [13], which later resulted in blindness ; he was friendly and courteous, receiving all strangers and attending to their maladies to the best of his abilities. He never left his desk and his books ; he was always either writing a work of his own or copying another author.

The problem that concerns us here is al-Rāzī's relation to alchemy and his place in the history of alchemy.

Al-Bīrūnī records 21 alchemical works by al-Rāzī [14]. Those works that have come down to us have been only imperfectly known [15] to Western scholarship, but recently the most important work on the subject of alchemy that was compiled by al-Rāzī has been translated by Prof. J. Ruska. This is the *Kitāb Sirr al-Asrār*, the *Book Secret of Secrets* [16].

The philosophical and scientific sources of this book can only be approached when it is remembered that al-Rāzī lived and worked in an age when the Islamic mind had attained its greatest consummation. He had the advantage of profiting by the work of extraordinary men who had come before him, who had, in fact, been the creators of a new culture. The roots of this culture can be found in late Hellenism, but more directly in that unique synthesis of Hellenistic and Eastern culture which developed in the great cities of Persia and Asia Minor [17], reaching as far as India, and after the exclusion of Byzantium,

[12] Mentioned by Hammer-Purgstall in *Literaturgesch d. Araber*, vol. iv, p. 358.

[13] See J. Ruska, *op. cit., infra*, [16], p. 5 and note 2.

[14] J. Ruska, *op. cit.*, iv, p. 47. See also J. Ruska, ' Die Alchimie ar-Rāzī's ', *Islam*, xxii, Berlin, 1935, pp. 281–319.

[15] Except the translations by H. E. Stapleton referred to by Prof. Partington in the following article. For printed and manuscript versions of the works of al-Rāzī known to the West prior to the translations of Ruska and Stapleton in the libraries of England and Ireland, see J. Ferguson, *Bibliotheca Chemica*, vol. ii, pp. 262–3 ; D. W. Singer, *Cat. of Latin and Vernacular Alchemical MSS. in Great Britain and Ireland*, Brussels, 1928–31 : vol. i, pp. 20, 53, 74, 79, 94, 100–111, 194, 218, 300 ; vol. ii, pp. 402, 511–36, 692, 708–9 ; vol. iii, p. 1163. For a detailed monograph concerning translations and versions of the *Kitāb Sirr al-Asrār* see J. Ruska, ' Uebersetzung u. Bearbeitungen von al-Rāzī's Buch Geheimniss der Geheimnisse ', in *Quellen u. Studien z. Gesch. d. Naturwiss. u. Med.*, iv, 3, Berlin, 1935.

[16] J. Ruska, ' Al-Rāzī's Buch Geheimniss der Geheimnisse ', *Quellen u. Studien a. Gesch. d. Naturwiss. u. Med.*, Berlin, 1937. For a discussion of the title of this work, see p. 31. From manuscripts other than the Göttingen one used by Prof. Ruska, as well as the title of the Latin translation, *Liber Secretorum*, it would seem probable that the true title of al-Rāzī's book was *Kitāb-al-Asrār* (*Book of Secrets*). The title *Kitāb Sirr al-Asrār* would more fitly be applied to a less voluminous treatise.

[17] See M. Plessner, *op. cit.*[3], *e*, and J. Ruska, *Tabula Smarag.*, cit.[2], *c*. See also F. Ueberweg, *Grundriss der Gesch. d. Phil.*, vol. ii, pp. 287–325, Berlin, 1928.

finding a greater home in Egypt and North Africa and in Spain. What we know of the philosophical outlook of al-Rāzī gives us certain clues to his mentality [18]. Al-Rāzī's theory of matter is contained in a work by the poet and Ismā'īlite missionary Nāṣir-i Khusraw [18a]. The Ismā'īlite system of thought [19] was based on a belief in the interaction of the macrocosmos and the microcosmos, and this was united with a scheme for social betterment—one of the reasons for the conflict of this sect with established authority—and furthermore there was a belief in strict secrecy (to divulge the esoteric doctrine of the sect was equivalent to adultery)—and also a belief in the presence on earth of the concealed Imam. The Ismā'īlites, moreover, took great pride in the transmission and preservation of 'Greek' science—this, in spite of their immersion in speculative and often fantastic ideas. They were in our sense 'humanistic', as were the Manicheans of the day, who believed in free learning as opposed to the obscurantism of the orthodox party. For this reason al-Rāzī was also called a Manichean [20].

To understand the mentality of al-Rāzī, therefore, we must recognize the fact that we find in him not only the greatest interest in empirical science, as demonstrated in his works on medicine, but also a mind influenced by a system of abstruse and symbolical metaphysics. Although Nāṣir-i Khusraw lived after the time of al-Rāzī, there is no reason to believe that the latter was not in active communication, or at least in sympathy with the Ismā'īlis [21]. As has been pointed out, so much in the writings attributed to Jābir [22] can only be correctly interpreted if the philosophical system of the Ismā'īlis is accepted as one of the sources of this group of alchemical texts. As the origin of the theory of Alchemy of al-Rāzī must be sought 'near' the works of the supposed Jābir [23], or in a source common to both groups of texts, the Ismā'īli philosophy is probably also one of the keys to the understanding of the more obscure part of al-Rāzī's alchemical writings.

Of all al-Rāzī's metaphysical theories only his theory of matter is of interest here [24]. He taught that there were five eternal principles (jauhar): the

[18] J. Ruska, *op. cit.* [16], p. 7 ff. S. Pines, *Beiträge zur islamischen Atomenlehre*, Berlin, 1936; P. Kraus and S. Pines in *op. cit.* [10], p. 34 ff. See also references to Rāzī in M. Horten, *Die philosophischen Ansichten von Rāzī und Tūsi*, Bonn, 1910; and M. Horten, *Die philosophischen Systeme der spekulativen Theologen im Islam*, Bonn, 1912.

[18a] Lived 1003–60. On his metaphysical work see *Encl. of Islam*, vol. iii, p. 870.

[19] P. Kraus, *op. cit.* [2], *i*, p. 30 ff. Article by W. Ivanow, *cit.* [2], *l.* See also W. Ivanow, *A guide to Ismaili Literature*, London, 1833. Also article by P. Kraus, *cit.* [2], *l.*

[20] H. H. Schaeder in M. Plessner, *op. cit.* [3], *e*, p. 105.

[21] P. Kraus, *op. cit.* [2], *i*, p. 30.

[22] J. Ruska & P. Kraus, *op. cit.* [2], *f*. See especially p. 16.

[23] P. Kraus & S. Pines, *op. cit.* [10]. The authors do not think that al-Rāzī was acquainted with the writings attributed to Jābir. Stapleton, however, is not in agreement with these views. *Memoirs of Asiatic Society of Bengal*, 1927, p. 335 *et seq.* See J. Ruska, *op. cit.* [16], p. 12.

[24] J. Ruska, *op. cit.*, p. 8.

Creator, the soul, matter, time, and space. Bodies were composed of indivisible elements and of empty space that lay between them. These atoms or elements were eternal and possessed a certain size. The characteristics of the four elements earth, water, air and fire, that is, their lightness and their heaviness, their transparency and colour, their softness and hardness, these characteristics were determined by the density of the elements, in other words by the measure of the emptiness of the spaces between them. These spaces determined the natural motion of the elements : water and earth moved downwards, air and fire upwards.

Even though al-Rāzī in his alchemy was not strictly empirical in our sense of the word, his great work mentioned above is a book of experiments ; it is a book of practical alchemy, and it rejects completely the speculative philosophy of the writings of the Jābir corpus [25]. Therefore it can be said that al-Rāzī is the creator of a new alchemy [26], for he seems to be the first to have transformed theoretical alchemy into a new and strictly scientific system. Or, to be more definite, al-Rāzī transformed alchemy for the first time into a science based on experiment. That accounts for the great vogue of his alchemical works, especially in the West : it was his scientific approach to the problems of nature that attracted the best minds for 700 years.

If the western student cannot understand the recipes given in *Kitāb Sirr al-Asrār*, it does not mean that they were written in order to delude the reader. The results of these recipes must not be thought of as the creations of a dishonest phantasy. The fact is that the system of concepts on which these experiments are based is totally different from that used in modern chemistry. Once the concepts of the older science are accepted it will be seen that the theoretical justification of that science is as valid as that of any system of scientific concepts in use to-day [27].

The basis of the alchemy of al-Rāzī [28] is explained in the next article (p. 192).

The purpose [29] of all the experimental work is the improving of base metals—the transformation of lead, tin, copper and iron into silver or gold, and the improving of ordinary stones, such as pebbles, glass, and ordinary crystals into red Yaqut, green emeralds and other precious stones.

The means by which these transformations [30] are to be accomplished is a powder or a liquid, produced according to a laborious method of work. This powder or liquid has an effect comparable to a strong medicine or a virulent poison when it permeates the base metals or the pulverized stones and transforms the whole substance into silver or gold, or into precious stones.

[25] *Op. cit.*, p. 13.
[26] *Op. cit.*, p. 3 ff.
[27] *Op. cit.*, p. 72.
[28] *Op. cit.*, p. 72.
[29] *Op. cit.*, p. 73.
[30] *Op. cit.*, p. 73.

The possibility of obtaining a species of matter which has these miraculous properties is based on the theory that all forms of matter contain within them a number of specific attributes, which are capable of being increased in potency to the highest degree of effectiveness, or, on the other hand, capable of being weakened or destroyed. The method by which this is accomplished consists, on the one hand, of adding other forms of matter with similar or opposite specific attributes to the substances with which one is experimenting, or, on the other hand, it consists of certain processes which are described by al-Rāzī in his book and which are difficult of comprehension [31].

If it is remembered that the characteristics of the four elements, according to al-Rāzī, are the attributes of earth, water, air and fire, and that these are the theoretical basis of this older chemistry, it is clear that the condition of a substance can be changed by means of the application of fire or water into its opposite, that is, the earthy condition can be changed into the liquid and airy, or the airy into the liquid and earthy. In addition to these basic characteristics some forms of matter also have the attribute of inflammability, which is due to the oiliness or sulphureousness of the matter. These two attributes can also be increased or diminished in potency according to the treatment of the matter. In the same way the attribute of salinity enters into this scheme, which is again capable of being increased or diminished.

Without doubt it is here that the origin of the popular conceptions of alchemy can be found, Mercurius, Sal and Sulphur, which reappear much later in Europe and play such an important part in the theory of western alchemy [32].

In the system of chemistry described by al-Rāzī, colours and the changing of colours are important. These occur when fire is applied to a substance or when the latter is mixed with another substance.

The spirits, according to the older chemistry, are the colouring and volatile substances.

Mercury [33], according to al-Rāzī, absorbed and removed moisture, Ammonium Chloride removed earthiness, Sulphur and Zarnīkh [34] produced whiteness and removed oiliness and inflammability. For producing redness all four spirits are applied, for whiteness especially yellow Zarnīkh. Working with Mercury also includes hardening, 'raising' (or sublimation), and amalgamating, while Ammonium Chloride is necessary for 'raising' and distillation, whereas Sulphur and Zarnīkh are necessary for 'raising', washing, roasting and boiling.

Al-Rāzī [35] defines calcination as depriving the bodies of their association

[31] *Op. cit.*, p. 73.
[32] *Op. cit.*, p. 73.
[33] *Op. cit.*, p. 74.
[34] Arsenic.
[35] *Op. cit.*, p. 74.

with one another and the burning of any sulphur or oil they may contain, so that they are transformed into white *Nuqra* [36].

There is no reference in al-Rāzī's work to the 'Stone' [37]. He uses 'ḥajar' only in the material sense when he means organic substances from which the elixirs are produced. An elixir is not only referred to as 'iksīr' [38], but also 'darūr' and 'habā'". The two latter probably mean a fine powder, with emphasis on the quality of 'fineness' [39]. More difficult has been the translation of technical terms that do not occur very frequently. For instance, 'ra's', pl. 'ru'ūs', 'head' or 'beginning', probably means 'elixir', but an elixir that has not quite reached its highest potency, although it can transform silver into a gold which does not lose its colour [40].

The word 'jauhar' is interesting [41]. It undoubtedly means 'essence' when used by al-Rāzī in a chemical sense, but it is also used for a prepared substance that can transform base metals into gold and pebbles into precious stones, a substance that has greater potency than the 'ra's'. Therefore we have here a new term for the 'lapis philosophorum'.

Another term, 'malāk al-amr', an acid-water, is called the 'basis of the work', because it removes the impurities from metals. The word 'nafs' [42] the dictionary meaning of which is 'soul', is used to describe a very valuable product of distillation, it is a substance that rises when the distilled water begins to change colour. The phrase 'distil its water until its nafs rises' occurs frequently. Mention is also made of hair, out of which white, red, yellow and black water is distilled as well as a 'nafs' and an oil.

Blood and the different parts of the egg [43] are used to produce liquids, oils and lime. According to Prof. Ruska it is a characteristic of the eastern school of alchemy that the greatest effects are ascribed to elixirs derived from organic substances.

The word 'ṭarḥ' [44] occurs, which is known to the west as 'projection'. Its effect is sometimes compared to the entrance of snake-poison into an animal body. Then there is the term 'tazwīj' [45], which can be translated as 'the nuptials' or 'pairing off'. It is perhaps used to describe a process whereby silver and gold are melted down together, but the strength of the elixir contained in the gold is sufficient to transmute the whole mass into gold. There are

[36] According to Prof. Ruska, translated at first by the European alchemists as 'pulvis intangibilis', *op. cit.*, p. 76.
[37] *Op. cit.*, p. 76.
[38] Prof. Ruska has not been able to give a satisfactory translation of this word.
[39] *Op. cit.*, p. 77.
[40] *Op. cit.*, p. 77.
[41] *Op. cit.*, p. 78. See also note [24].
[42] *Op. cit.*, p. 79.
[43] *Op. cit.*, p. 79.
[44] *Op. cit.*, p. 79.
[45] *Op. cit.*, p. 79.

also other meanings of this term which are not so easy to comprehend, as, for instance, combinations of substances of a different order.

The potency of the individual ' iksīr ' may vary very much [46]. According to al-Rāzī, if one ' iksīr ' transmutes a metal one hundred or one thousand times, its strength is medium ; the more potent ' iksīr ' is supposed to be able to transmute several thousand times, and there is even a mention of 1 in 20,000, this effect being produced by the most powerful ' iksīr ' described by al-Rāzī.

The problem of how to approach this book must depend upon the realization that the chemistry described here is only the last remnant of a system of thought that was once spread over a great part of the civilized world. And perhaps a critical remark, written on the margin of the manuscript of this book, might not be amiss here : Abu'l-Qāsim al-Muqaddas says : 'Allah have mercy on him. Truly I have looked into this book . . . . Do not occupy yourself with them (*i. e.* the essences of Zarnīkh and of Sulphur) unless you already know the secret of the process. . . . Only if you know the secret, so God be willing, will you accomplish the work [47] '.

[46] *Op. cit.*, p. 80.
[47] *Op. cit.*, p. 82.

THE CHEMISTRY OF RĀZĪ.

By J. R. Partington, M.B.E., D.Sc.

The following summary has been prepared mainly from two sources [1].

Materials (Syriac *'eqārā*, pl. *'eqārē*, 'roots'; Arabic *'aqqār*, pl. *'aqāqīr*; the name *arkān*, pl. of *rukn*, pillar, is used for 'principles') belong to three classes, viz. animal, vegetable and mineral. The mineral kinds comprise six groups:

1. Four spirits (*rūḥ*, pl. *arwāḥ*), viz. two volatile and combustible, sulphur and arsenic (*i. e.* the sulphides); and two volatile but incombustible, mercury and sal ammoniac (*nūshādur*). The first three are given by Zosimos, who also adopts the classification into spirits and bodies (metals).
2. Seven bodies (*jasad*, pl. *ajasād*), viz. the metals gold, silver, iron, copper, tin, lead and *khar ṣini*. The number seven, corresponding with the planets [2], goes back to Greek sources, but *khar ṣini* has replaced electrum.
3. Thirteen stones (mostly minerals and gems, but also glass).
4. Five vitriols (or six if alum is included).
5. Six boraces.
6. Eleven salts.

The name used for mercury (*zībaq* or *zaibaq*) is Middle Persian and derives from a root meaning 'life', preserved in our name 'quicksilver'. The Arabic form *al-zāwūq* appears in the Latin translations as *azoc*, *azoth*, etc., and the Spanish *azogue*. Several old Persian and Arabic authors mention deposits of mercury ore (cinnabar) in Persia. The metal was, apparently, little known or used.

The origin of the name for sal ammoniac (*nūshādur*) is obscure and the material is, apparently, first certainly described by Persian or Arabic authors [3]. It is called by Rāzī *al-'uqab*, 'the eagle', on account of its volatility, and was

[1] *A*. 'Chemistry in 'Iraq and Persia in the Tenth Century A.D.', by H. E. Stapleton, R. F. Azo and M. H. Ḥusain, *Memoirs of the Asiatic Society of Bengal*, **8**, no. 6, pp. 317–418, Calcutta, 1927.

B. 'Al-Rāzī's Buch Geheimnis der Geheimnisse, mit Einleitung und Erläuterungen in Deutscher Übersetzung', by Julius Ruska, *Quellen und Studien zur Geschichte der Naturwissenschaften und der Medizin*, Band 6, Berlin, 1937.

[2] Partington, *Ambix*, 1937, **1**, 61.

[3] On sal ammoniac see Laufer, *Sino-Iranica*, Chicago, 1919, 503; Ruska, 'Sal ammoniacus etc.', *Sitzb. Heidelberger Akad. der Wiss., Phil.-hist. Klasse*, 1923, Abh. 5; *ibid.*, *Z. angew. Chemie*, 1922, **25**, 719; 1928, **41**, 1321; *ibid.*, *Das Steinbuch des Aristoteles*, Heidelberg, 1912, 43; Stapleton, 'Sal Ammoniac etc.', *Mem. Asiat. Soc. Bengal*, Calcutta, 1905, **1**, no. 2, pp. 25–42; Abulfeda, *Géographie*, transl. Reinaud, Paris, 1848–83, i, 372; Berthelot, *Archéologie et histoire des sciences*, Paris, 1906, 262.

perhaps obtained from the burning coal deposits of Central Asia ; Rāzī says a mineral kind came from Khorasan and Samarkand, but these were probably only markets, and the volcanic sources of Demawend, Kirmān and Etna were probably not known at that time. A second kind was obtained by the distillation of hair ; this would be impure ammonium carbonate, not the chloride.

Arsenic (*zarnīkh*) comprised red realgar (As_2S_2), the best kind ; yellow orpiment (As_2S_3) ; and a grey kind. The name is probably derived from the Persian *zär*, ' gold '. Realgar and orpiment were known from early times [4]. Rāzī also uses the cover-name *al-'alam*, ' the flag ', for them and was well acquainted with their properties, *e. g.* their ability to make copper white, as described in Greek treatises.

Sulphur (*'aqrab*, ' the scorpion '), of which a red form—unknown to the vulgar and with wonderful properties—a white form, a black form (contaminated with bitumen) and a crystalline form like glass, are described, was much used.

The metals are given either the common or the planetary names. The Chinese metal *khar ṣini* has been variously identified ; it may be an alloy of copper, zinc and nickel [5], or metallic zinc [6].

The thirteen ' stones ' (minerals) are *marqashīṭā* (pyrites of various colours), *maghnīsiyā* (sometimes manganese, *i. e.* native pyrolusite, MnO_2), *dawṣ* (an ore of iron ?), *tūtiyā* (various light-coloured minerals, perhaps including zinc ore), *lāzaward* (*lapis lazuli*, or the copper mineral azurite), *dahnaj* (malachite), *fīrūzaj* (turquoise), *shādanaj* (' bloodstone ' ; hæmatite ?), *shakk* (arsenic, white and yellow, including sublimed arsenic oxide, As_2O_3, mixed with sulphide, which Rāzī thought was a natural product), *kuḥl* (galena or stibnite [7]), *talq* (mica or selenite ; talc ?), *jibsīn* (gypsum ; Arabic *jaṣṣ*, *jiṣṣ*), glass (*zujāj*, the best being Syrian like crystal, *billaur*), Egyptian frits (*mīnā*) for imitating gems, and various gem stones.

The varieties of vitriol (*zājāt*) are *qalqant* (= χάλκανθος), *qalqadīs* (=χαλκῖτις), which is white and was perhaps alum ; *sūrīn* or *sūrī* (=σῶρυ), which is described as red ; and *qalqaṭar* (=colcothar) ; all these are imperfectly described, blue vitriol not being mentioned. Alum (*shabb*) is classed with the vitriols but is not well described.

The six ' boraces ' include *naṭrūn*, used by the baker (sodium sesquicarbonate) ; *tinkār*, used by the goldsmiths and perhaps our borax, although a product crystallized from six salts is described ; borax of Zarāwand ; and *būraq al-gharb* (acacia gum ?, Stapleton ; willow ash, Ruska).

The (eleven) salts are ' good ' salt (' pleasant to the taste ', probably refined common salt), bitter salt (perhaps Epsom salt or Glauber salt), rock salt

[4] Partington, *Origins and Development of Applied Chemistry*, London, 1935, index, 584, 588.

[5] Laufer, *Sino-Iranica*, Chicago, 1919, 555.

[6] Stapleton, *op. cit.*, 1927, 407.

[7] *Cf.* Partington, *Origins*, index, *mestem* and *kuḥl*.

(*ṭabar-zad*; crystallizing like sugar candy), salt of Andarānī, naphtha salt (from petroleum wells), Indian salt, *qali* or *alqilī*, the purification only of which is described (probably impure carbonates of potash and soda from the combustion of a plant *ushnān*, salsola, salicornia, etc. [8]), *nūra* (slaked lime), salt of urine (perhaps very impure microcosmic salt ?), salt of oak ashes, and ' salt of egg ' or ' salt smelling of boiled eggs '.

Materials artificially prepared (' derived ') are litharge (*martak*; Mod. Persian *murdeh säng*, ' dead stone ') obtained by roasting lead; red lead (*usrunj*; *zarqūn*); white lead (*isfīdāj*); iron oxide (*za'faran al-ḥadīd*, the alchemist's ' saffron of Mars '); *iqlīmiyā* or *qalmiyā* of gold and silver (probably litharge from the cupellation of these metals, although yellow and white calamine, or zinc oxide products, are possible); copper scales (*rūsukhtaj*; Persian *rūy sukhtah*, burnt copper); verdigris (*zanjār* or *zinjār*); zinc oxide (?; *tūtiyā*); iron scales (*dawṣ*); arsenic oxide (*shakk*); glass-gall (*masḥaqūniyā*); arsenical copper (*ṭālīqūn*); *theion hudor* (*zād al-Raghwah*; by boiling sulphur and slaked lime with water); cinnabar (*zunjufr*; obtained by subliming mercury and sulphur, and in the wet way from mercury and *theion hudor*). Many other products are mentioned incidentally, *e. g.* a red sublimate from vitriol and sal ammoniac, which would contain ferric chloride.

Rāzī says vegetable products are little used in alchemy and only in one way (distillation ?); the calcination to produce potash is not described, although ashes of many vegetables (galls, oak wood, willow wood) are mentioned. The drugs *'anzarūt* (=σαρκοκόλλα), aloes, *ḥilīt* for insect powder, mustard for purifying mercury (perhaps by separation of lead as sulphide ?), honey, date syrup (*dūshāb*), and wine vinegar, are mentioned in recipes.

Animal matters are not much used, except ' sal ammoniac of hair ', made by distilling hair (and probably impure *sal volatile*), urine, and the curious *shīzraq* or *shīrzaq*, bat's guano, which is frequently used.

Apart from borax (?) and sal ammoniac, all the materials mentioned were known to the Greek alchemists, and Rāzī added little to the chemical knowledge of substances, although he appears to have been acquainted with caustic soda and perhaps crude glycerine (Stapleton, *op. cit.*, 1927, 391, 393).

The apparatus used by Rāzī included files to prepare filings (*burāda*) of metals, metal spatulas (*ṣafa'iḥ*) or plates for heating (probably the Greek κηροτακίς), the hammer (*māshiq*; Stapleton read *māshū*, ladle), shears (*miqta'*), tongs or tweezers (*māsik*), muller (*miksar* or *mukassir*), ' marble ' or flat stone (*ṣīlāya*), roller (*fihr*; the mortar, *hāwūn*, is not specifically mentioned) for crushing substances to powder on the flat stone, ladle (*mighrafa*), crucible (*būtaqa*), double crucible (*būṭ eber būṭ*, the upper perforated), mould (*rāṭ*), duster (*khirqa*), bag (*ṣurra*), and roasting pan (*miqlā*). Various degrees of heat (temperature) are specified, from that of a sitting hen to that of a furnace. These are applied by means of the candle or lamp (*qindīl*), wax candle or torch

[8] *Cf.* Partington, *Origins*, 498 f.

mash'al or *sham'a*), naphtha lamp (*naffāṭa*) and various types of furnace (*atūn*). These are the brazier or chafing dish (*ṭabistān*), the oven fire (*tannūr*, from which through *al-tannūr*, pronounced *at-tannūr*, comes the alchemist's 'athanor'), the smelting furnace (*kūr*), and the wind furnace (*nāfikh nafsahu*) with a perforated base and supported on a tripod. Bellows of soft leather were used.

Glass apparatus included the beaker (*qadaḥ*, pl. *aqdāḥ*; Stapleton gives the Persian *jām*, but *cf.* below), flask (*qāuqūra*, pl. *qawārīr*), phial (*qinnīna*, pl. *qinānī*), sugar basins (*sukkurajen, sukrujen*), and glass crystallizing dish (*jām*). Of earthenware were the jug (*kūz*, pl. *kīzān*), wine jug (*dann*), casserole (*tinjīr*), washing mug (*ijjana* or *injana*), pot with lid (*durj*), and porous cooling pot (*sirdāb*; this name was used for deliquescent substances). A clay lute for glass vessels is *gharḍar*, made from clay, rice, salt and chopped hair (luting glass vessels with a 'skin' is described by Zosimos[9]), or a 'lute (*ṭīn*) of the philosophers'. A soft stone (*birām*) was also used for vessels. The aludel (*uthāl*), although very often used, is not very clearly described. The parts of the alembic are the *qar'a* ('gourd' or cucurbit, Greek λωπάς), and *anbīq* ('capital', Greek ἄμβιξ; with the article, *al-anbīq*, this became 'alembic'). The receiver was the *qābila*. The head or upper pot was set in a groove (*khandaq*) in the broad rim of the cucurbit or lower pot. Filtration is not very clearly described, that 'with a wick' being by capillary attraction through a wick hanging over the edge of a vessel ('distilling through a filter' in Geber). Weights and volumes are often specified but there is no description of the balance or liquid measures. The *raṭl* (=λίτρα) is usually a liquid measure of about 1 to 2 litres, but was perhaps a unit of weight, divided into 12 ounces (*ūqiyya*) of about 30 grams (*sic* Ruska; $12 \times 30 = 360$ grams, whilst 1 litre =1000 grams). The *mithqāl* is 6 grams and the drachm (*dirham*) is 4 grams. The quantitative aspect of experiment is almost entirely absent, weights being specified only in recipes.

Operations include the calcination (*taklīs*), burning (*taḥrīq*; of filings mixed with sulphur, arsenic or sal ammoniac), amalgamation (*ilghām, talghīm*), and rusting (*taṣdiyya*) of metals; washing (*ghusl*), purification (*taṣfiyya*)—including on the filter (*rāwūq*)—refining (*ṣaffa*), solution (*ḥall, taḥlil*)—including that of substances insoluble in water by means of 'sharp waters', the process also being called 'melting', the vessel being heated in a dung bath, water bath, etc.—evaporation (*ta'rīq*), boiling (including boiling in oil till it becomes black), maceration (*tashmī'*; Latin *ceratio*)—a process applied to various materials, including 'spirits', and probably in many cases involving the production of fusible salts or pastes composed of oils and salts, and mostly useless chemically—coagulation (*'aqd*), distillation (*taqṭīr*) in the alembic, sublimation (*taṣ'īd*) in the aludel, and 'choking' (*takhnīq*) by sublimation in a flask with a narrow neck until the latter became stopped. Sublimation of mercury with vitriol and salt[10] gave what was probably corrosive sublimate.

[9] Berthelot, *Coll.*, iii, 229, 240.
[10] Ruska, 1937, p. 104.

The ' sharp waters ' include caustic soda but not mineral acids. Vinegar, sour milk, and lemon juice were the only acids used. The ' sharp vinegar ' was sour milk and lemon juice. ' Virgin's milk ' (*laban 'al-aḍra* ; already mentioned by the Greek alchemists, for whom it meant mercury) may be an acid liquid. Some of the ' sharp waters ' contain sal ammoniac (which has a violent action on most metals) with slaked lime (*nūra*), alkali (*qali*), bat's guano and vinegar.

It will be seen that the apparatus and operations described by Rāzī show little advance on those described by the Greek alchemists.

The operations described by Rāzī are generally obscure and difficult to interpret chemically. They frequently involve the use of ' incompatibles ', *e. g.* acidic and alkaline materials mixed in the same preparation, probably because their ' virtues ' are thought to be conserved ; and non-volatile materials are directed to be sublimed. Typical specimens are given by Stapleton etc., and all are translated by Ruska.

According to Dr. Holmyard [11], no Muslim chemists are outstanding except Jābir and Rāzī. It has been claimed [12] that the writings ascribed to Jābir are later than those of Rāzī, and if this is the case the importance of Rāzī in the history of chemistry is increased. In the introduction to Rāzī's treatise *Kitāb al-Asrār* in the Escorial manuscript, however, Jābir is referred to by the writer as his master.

[11] *Scientia*, 1926, **40**, 290.

[12] Kraus, *Encyclopædia of Islam*, Suppl. i, 1934, p. 52.

AN ALCHEMICAL JOURNAL OF THE EIGHTEENTH CENTURY.

By Gerard Heym.

[Plate IX.]

A weekly journal devoted exclusively to their subject was one of the delights enjoyed by the alchemists of the eighteenth century.

The bound volume of the journal before me is prefixed by the following announcement :

> Der Chymische Warsager,
> Oder eine in zwanzig Wochen-Blättern herausgegebene Unterweisung,
> Worin der grade Weg zum Brunnen des Lebens in der Natur, und zu desselben concentrierten Kraft-Wesen/ insgemein LAPIS PHILOSOPHORUM gennannt,
> Aufrichtig angewiesen, und denen so die Wahrheit lieb haben, deutlich endecket wird.
> Nebst einer kurz abgefassten gründlichen Vorrede,
> von
> A.C. CCC. S.F. EEE. P.H. JJJ. N.K. RRR. L.A.
>
> Hamburg,
> Gedruckt mit Benens Schriften, im güldenen ABC. 1748.

There follow three pages of printed text, beginning with the heading *A* *O*, the last page containing only seven lines on the verso.

This announcement purports to describe the art of true alchemy. The writer urges ' qualified seekers ' by all means to buy the *Chymische Warsager*, as the ' path is slippery and deceptive '. He continues his advice by insisting that the preparation of the Stone is easy, provided that it has been first manufactured ' in the head '. He also says that the Great of this world will hardly be able to obtain the Stone, as they are interested in too many ' worldly distractions '. Then he bids farewell to the reader, and the remainder of the last page contains a pretty vignette showing a winged genius pointing to what is supposed to be a Hebrew cabalistic arrangement of letters, from which rays are emitted.

No doubt the author and publisher of this journal were left with a good many sets. Let us hope that this announcement, an example of alchemical salesmanship, drew the attention of many Sons of the Art to the bound copies of the journal, which were quietly reposing on the booksellers' shelves.

The title-heading of the first number of the journal is illustrated in Plate IX. It begins :

Der
Chymische Warsager.
Sic est vulgus, ex veritate pauca, ex opinione
multa æstimat. *Cic. Pro. Rosc. Com.*
No. I. den 11 Nov. 1747.

Underneath the last two words there is a vignette of a flying angel, clothed in long robes, blowing a trumpet and hovering between two pillars (Plate IX). At the side of this device the text begins and comprises two sheets printed on both sides. The format of the journal is quarto, and every number except the last is of the same length, the last number comprising four printed sheets. At the end of the text, on the last page, most numbers have a notice to the effect that ' this weekly is distributed on Saturdays .... Each copy costs 2 Schilling '. Instead of this notice a few numbers have a list of errata.

In the first number the author endeavours to justify the publication of his opus. He says that ' those who have already a solid conception of what true chemistry really means, will welcome this paper with joy and fear. . . . With joy, because here they will find the key .... to the real essence of Nature and how to obtain it (*i. e.*, the essence and the true meaning) and how to perfect it. . . . With fear because they may think that here is another false alchemist, a rogue and a seducer, or they may think that a possessor of the secret will give them quite as many dark and unintelligible hints for the making of the so-called Philosopher's stone as all others have done. This worry, however, is unnecessary, as a *Chymische Warsager* (truth-teller) must tell the truth ....' Again, ' The *Warsager* could tell the truth in 10 numbers instead of 20, but that would be too easy an opportunity for thousands of people of brutal temperament to enrich themselves .... and it would only bring unhappiness '.

The second number appeared on November 18th, 1747. The heading is the same, except that we have a different Latin quotation and no vignette, which only appeared in the first number. This second number is devoted to one of the usual alchemical tales of a secret book, a vial containing a liquid, twelve mirrors, and a powder. It concerns an incident in the life of a Hamburg adept, the late Herr Jesse.

No. 3 describes an alchemical operation in the language common to eighteenth-century German books on the subject, and in no. 4 we have a story of an old recluse who had seen the first number of the *Warsager*. There follows an allegorical tale of a symbolical mountain, which is continued in the next number. No. 6 describes another alchemical incident and quotes a long poem. In no. 7 an alchemical process is described by the lavish use of allegory, while in nos. 8 and 9 there is an exhortation to the pious to remember the

Der Chymische Warsager.

Sic est vulgus, ex veritate pauca, ex opinione multa æstimat. *Cic. pro Rosc. Com.*

No. I. den 11 Nov. 1747.

Vorbericht.

Wehrte Leser!

Dieser Warsager wird zwar nur einmahl wöchentlich sich sehen lassen; ich besorge aber, daß er bey denen jetztlebenden Weltbürgern eben keinen gar zu geneigten Beyfall finden wird. Denn einmal hat ein jeder vernünftiger Mensch von Natur einen innerlichen Abscheu vor Warsagern, Zeichendeutern, Hexenmeistern u. d. g. und dieses ist auch so löblich als billig. Denn wer wollte mit dergleichen Teufelsgeschmeiß Gemeinschaft haben; in einer solchen fürchterlichen Gestalt wird

Title-page of *Der Chymische Warsager*, No. 1.

teachings of the Bible, which excellent advice is followed by an interesting dissertation on salt.

With no. 10 the reader is getting down to his subject. Two stories of ' adepts ' are told, those of Böttger and Starkey, to serve as a guide to the reader, after which comes another dissertation on salt. But in the remaining numbers there are interesting descriptions of the alchemical process, rather in the manner of the *Aurea Catena Homeri* ; the style of this part of the journal may be said to be logical, and considered as symbolical alchemy these numbers are good pieces of writing. The last number, no. 20, which appeared on March 23rd, 1748, requests the reader at length to acquire the right frame of mind for the Great Work, if he wishes to succeed. Finally we have a long poem, said to be by Sendivogius, and describing the process in detail ; this poem is by far the most interesting and readable part of the journal.

We know that the journal was actually read, for after the fourteenth number a certain Peter Schmiedeknecht wrote a dissertation of six printed pages in the same format. It is entitled

> Hrn. P. Schmiedeknechts
> Philosophischer Traum und Beurtheilung
> über den in Hamburg edirten
> Chymischen Warsager, ...
> den 26 Febr. 1748.

It seems Herr Schmiedeknecht was worried. He did not know whether or not the author of the *Warsager* possessed the stone. Therefore he had a dream in which the whole secret was revealed to him—an allegorical romance with angels, like the *Chemical Nuptials* and well written—and he thought that the readers of the *Warsager* ought to profit by his dream. Therefore he sent it to the publishers, hoping they would publish it. Which they did.

Note.—For alchemical Journals, see Kopp, *Die Alchemie in älterer und neuerer Zeit.* Heidelberg, 1886, ii, p. 343.

The *Wahrsager* mentioned by Kopp, *op. cit.*, ii, p. 349, is another work, which was published in 1755.

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